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THE PSYCHOLOGICAL REVIEW

PRESENT POSSIBILITIES FOR AUDITORY THEORY

BY ERNEST GLEN WEVER AND CHARLES W. BRAY

Princeton University

Logically it has always been evident, but modern physiological developments have now made it imperative, that a theory of hearing to be entitled to serious consideration must explain intensity as well as pitch. Increasing knowledge of the nature of the conduction process in nerve has been the determining factor here; the character of the sensory transmission process imposes a final and decisive limitation to the effective response characteristics that can be attributed to the end-organ.

At the time that Helmholtz, stimulated by the specific energy doctrine and the mechanical process of sympathetic vibration, formulated the theory of hearing that has led a controversial field for more than half a century, the physiology of nerve was in its infancy. It was the obvious thing, after representing pitch by the particular nerve fibers actuated in a given case, to correlate intensity with the strength of the nerve processes involved. It was so obvious, indeed, that Helmholtz hardly more than hinted at the correlation.[1]

Subsequent writers, extending from Helmholtz's day almost to the present, must have perceived the same obviousness, since they for the most part have expended their effort upon pitch, and have ignored the matter of intensity. Adherents to the outstanding rival of the Helmholtz resonance hypothesis, the so-called telephone theory of hearing, have sinned as much as any in this omission. By them also, after pitch was accounted for, in their case by frequency of impulses

[1] H. Helmholtz, Sensations of tone, Ellis' trans., 1885, p. 148.

in the auditory fibers acting as a whole, intensity was usually passed over, or if mentioned at all, correlated with strength of nerve response.[2]

Influence of the All-or-None Principle.—With the discovery, first in motor and later in sensory nerve fibers, of the all-or-none characteristic of nerve response,[3] the matter of intensity in audition came into prominence. If a nerve fiber, once stimulated, always responds with a vigor that is a function of its own condition and not of the strength of the stimulus, then the simple, straightforward explanation of intensity is cut away, and the adherents to both the resonance and the telephone types of theory are left with an essential part of their system in ruins.

Fortunately, however, reconstruction is possible. Two modes of nerve response still remain; these are the particular fibers responding in a given instance, and the frequency of response of these fibers. It is possible, therefore, for each of the theories to retain its old explanation of pitch, and ascribe the other mode of response to intensity. The alternative character of the rival hypotheses is thereby made manifest. The place-resonance theory retains particularity of fibers for pitch, and correlates intensity with the frequency of impulses in these fibers; the telephone theory, on the other hand, correlates frequency of nerve impulses with pitch, and explains intensity in terms of the numbers of fibers involved in a given instance.

The Resonance Theory.—Forbes and Gregg, in 1915, were first to point out this implication of the all-or-none principle for auditory theory, and to recast the Helmholtz resonance theory in accordance with this principle.[4] Most later resonance formulations have followed the outlines of their position, differing only in detail. This modern form of the Helmholtz

[2] *Cf.* W. Rutherford, A new theory of hearing, *J. Anat. and Physiol.*, 1887, 21, 166–168.

[3] The all-or-none principle began to be accepted for motor nerves about 1912, and though Forbes and Gregg applied it to sensory nerves as early as 1915, it was not established in them before the recent investigations of Adrian and his collaborators. See, for a discussion of this subject, A. Forbes, 'The mechanism of reaction,' in The foundations of experimental psychology, ed. by C. Murchison, 1929, pp. 128–168.

[4] A. Forbes and A. Gregg, *Amer. J. Physiol.*, 1915, 39, 229 *ff.*

theory can therefore be stated as follows. Pressure-variations from a sound-source are transmitted to the cochlea, and there excite limited portions of the basilar membrane through resonance. Since the membrane is graduated in its properties, the various frequencies are spread over its linear extent. Analysis of complex wave-forms is thus a peripheral process. Place of excitation on the basilar membrane is correlated with frequency of stimulation, and since the separate portions of the membrane are served by separate nerve fibers, the correlate in the conduction process of stimulus frequency, and hence of auditory pitch, is the particular nerve fibers excited.

Intensity is correlated in the theory with number of impulses in the nerve response. A greater number of impulses may be secured by an increase of impulse frequency in the fibers concerned, or by an increase in the number of fibers acting, or by both these methods. The first method assumes, as seems likely, that greater amplitude of sound-waves produces a greater amplitude of movement of the region of basilar membrane involved. Amplitude of movement then is translated into excitation process, and the increased magnitude of this process is supposed to lead to an increased frequency of response in the nerve fibers concerned.

The second manner of accounting for intensity assumes that greater intensity of sound-waves involves a greater spread of response on the basilar membrane, and hence a greater number of nerve fibers excited.

It should be added that the development of the concept of spread of resonance on the basilar membrane has led to the difficulty of explaining how any particular designation of pitch is possible. In the face of this difficulty Gray [5] has appealed to a principle which he called the principle of maximum stimulation, which states that the pitch of a tone is determined by that part of the membrane in which the stimulation is greatest. In the nerve fibers, as suggested

[5] A. A. Gray, On a modification of the Helmholtz theory of hearing, *J. Anat. and Physiol.*, 1900, 34, 324–350. Hartridge, it may be noted, denies Gray's principle, and does not see that there is any problem: see 'Hearing,' in Foundations of experimental psychology, ed. by C. Murchison, 1929, p. 343.

above, this maximum may be represented in terms of maximum frequency of response.

The Telephone Theory.—The telephone theory has received its most modern formulation in the hands of Boring.[6] Frequency of response in the nerve is correlated directly with the frequency of sound-waves. Greater amplitude of sound involves increased spread of excitation on the basilar membrane, and hence includes a greater number of nerve fibers in the conduction process. Analysis in this theory is not a peripheral but a central affair.

The theory is able to explain binaural localization in terms of phase difference, as the resonance theory stated above does not do; for in the Helmholtz theory, if we have peripheral analysis of the form described we have no longer a means of transmitting to some central relating mechanism the phase differences of the tones applied to the two ears. In the telephone theory, on the contrary, the tones are transmitted centrally at their proper frequencies, and consequently in their incident phase relations; a phase difference can therefore be appreciated directly. This matter of binaural phase localization provides, therefore, one of the most effective arguments for the telephone theory as against the peripheral analysis theory.

The remaining points of difference between the two types of theory just outlined may be reduced to three: the question of peripheral versus central analysis, the intensity-frequency correlation in nerve response, and the matter of refractory phase in nerve. The evidence regarding these points seems at the present time to favor the resonance type of theory.

Analysis.—The matter of analysis without a doubt favors the resonance theory; it is just what that theory is devised to explain. But the proponent of the telephone hypothesis need not be completely abashed: he can weigh the advantages of representing a tonal complex in terms of cortical locus, or of representing the complex in terms of the temporal relations of impulses communicated from periphery to center, and he can then flippantly inquire whether we have reason to believe

[6] E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, *Amer. J. Psychol.*, 1926, 37, 157–188.

that the 'soul' is any better geographer than mathematician. It is a very real problem whether there is much to be gained in the spatial distribution of coincident frequencies which constitutes peripheral analysis.

The Intensity-Frequency Principle.—Recent investigations of sensory nerve responses by Adrian and others [7] have shown that for pain, pressure, vision, and muscle-sensitivity intensity of stimulation is correlated with frequency of nerve response. These findings provide a very powerful support for the modern formulation of the resonance theory of hearing, as it seems reasonable to assume that the intensity-frequency relationship found in these other sensory fields holds also in audition. Yet the evidence is extrapolated from the other sensory nerves, and it is plain that a final decision rests upon investigation of the auditory nerve itself.

Refractory Phase.—Perhaps the most frequent attacks upon the telephone theory have been based upon considerations of refractory phase in the auditory nerve fibers. The theory demands a rapidity of response in the auditory fibers in the neighborhood of 20,000 ~/sec. in order to account for the highest tones audible. For a nerve fiber to attain this frequency its absolute refractory phase must be no greater than 0.05 ms.[8] Yet most determinations of refractory phase in mammalian nerve have given values of the order of 1 ms., and the smallest figure so far obtained is 0.6 ms.[9] A refractory period of 0.6 ms. would allow a maximum frequency of only 1667 ~/sec., less than a tenth of the required value.

Moreover, recent evidence goes to show that nerve fibers during continued stimulation suffer an increase in their refractory periods; [10] and on a telephone theory this might

[7] E. D. Adrian, The impulses produced by sensory nerve endings, Part 1, *J. Physiol.*, 1926, 61, 49–72; E. D. Adrian and Y. Zotterman, Part 2, The response of a single end-organ, *ibid.*, 1926, 61, 151–171; E. D. Adrian and Y. Zotterman, Part 3, Impulses set up by touch and pressure, *ibid.*, 1926, 61, 465–483; E. D. Adrian, Part 4, Impulses from pain receptors, *ibid.*, 1926, 62, 33–51.

[8] ms. (millisecond) = 0.001 sec.

[9] H. S. Gasser and J. Erlanger, The nature of conduction of an impulse in the relatively refractory period, *Amer. J. Physiol.*, 1925, 73, p. 629. The value given is the absolute refractory period for the phrenic nerve of the dog.

[10] H. Field and E. Brücke, Ueber die Dauer des Refraktärstadiums des Nerven bei Ermüdung und Erholung, *Arch. f. d. ges. Physiol.*, 1926, 214, 103–111. See, also A. Forbes and L. Rice, Fatigue in peripheral nerve, *Amer. J. Physiol.*, 1929, 90, 119–145.

be expected to mean that a high tone if sustained should decline gradually in pitch, as seems not to be verified in experience.

The argument against the telephone theory rests, therefore, upon the expectation from anatomical resemblances that auditory nerve fibers possess properties essentially similar to the nerve fibers investigated. Many are unwilling to believe that auditory nerve fibers operate on an order of frequency 10 or 20 times higher than other nerves which they closely resemble. Others, upholding the telephone hypothesis, point to the known differences between nerves found in a single organism, and suggest the possibility that the auditory nerve may have special properties.

So the dispute goes on; the available evidence is not conclusive, and only a direct measurement of the response characteristics of the auditory nerve in an animal whose upper threshold of hearing is known will lead to a final decision.

Experiments on Auditory Nerve Responses

By reason of the recent rapid development of physical resources and physiological technique, we are now in a position to look for some such measurement as just suggested. A pioneer investigation of action currents in the auditory nerve in response to acoustic stimulation was made by Buytendijk in 1910.[11] He was able to obtain galvanometric deflections coincident with stimulation of the ear of the rabbit and the guinea pig by loud sounds such as firing off a pistol and blowing a flute. No differentiation of the responses was possible, and the results, though of much interest from the point of accomplishment of a difficult experiment, do not aid in the field of auditory theory.

A more recent attempt in this field was made by Forbes, Miller, and O'Connor,[12] following Boring's suggestion, to secure physiological data on auditory nerve responses of a

[11] J. J. Buytendijk, On the negative variation of the nervus acusticus caused by a sound, *K. Akad. Wetensch., Amsterdam*, Proc. Sci. Sect., Eng. trans., 1911, 13, 649–652.

[12] A. Forbes, R. H. Miller, and J. O'Connor, Electric responses to acoustic stimuli in the decerebrate animal, *Amer. J. Physiol.*, 1927, 80, 363–380.

sort calculated to throw light on the theoretical matters in question. Experiments were performed on the decerebrate cat by placing electrodes on the brain-stem or medulla and recording on a string-galvanometer the impulses resulting from stimulation of the ear by sound. Unfortunately for the purposes of the experiment, the string-galvanometer used was not capable of responding at frequencies high enough to give any crucial evidence for auditory theory. The limit of response of the instrument the writers state as about 500 per second. It therefore could not operate satisfactorily except at very low frequencies; actually, the experiment revealed responses only up to about 200 per second. These the authors believed were probably synchronized with the stimulus frequency, though, since the stimulus could not be recorded simultaneously with the nerve response, there was no certain way of demonstrating it.

The authors concluded in favor of the resonance-place theory of hearing; the apparent correlation of sound frequency and response frequency around 100 per second as shown in the records does not lead in their opinion to a telephone hypothesis. They pointed instead to the possibility that at these very low frequencies, well within the frequency ranges shown in other nerves, the various nerve fibers involved in the response may be going off more or less in synchronism with the points of maximum amplitude in the sound waves. Actually, then, the frequency correlation is with intensity of excitation, though at first glance it seems to be with excitation frequency because only responses to low tones are measured.

The situation outlined is about as illustrated in Fig. 1. Any given fiber, as *a*, responds at each maximum of the wave with a straggling series of impulses. If several fibers are operating the result is about as pictured at the bottom of the figure.

In substance, this theory of Forbes, Miller, and O'Connor is no more than the older Forbes-Gregg formulation of the Helmholtz theory with a minor suggestion to take care of the special case of sound frequencies well below the maximum response frequencies of the nerve fibers. The only notable

difference from the earlier theory is the greater emphasis upon the supposition that a single stimulation may give rise to a repetitive discharge in a nerve fiber.

Another investigation of auditory nerve responses, also stimulated by Boring's suggestion, is now being carried on by the present writers. The general nature of the investigation

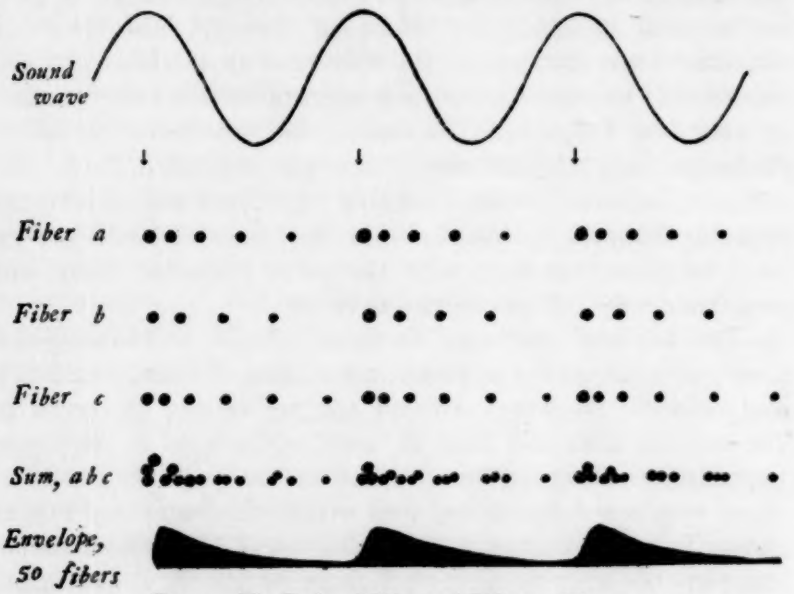

FIG. 1. The Forbes, Miller, and O'Connor Theory

Each fiber responds with a series of straggling impulses. A number of fibers might give an envelope, as recorded on a galvanometer, with maxima corresponding to the sound wave frequency.

resembles fairly closely that of Forbes, Miller, and O'Connor just reported; the cat is used as the experimental animal, and action currents from the auditory nerve are led off through an amplifying system to a recording instrument. The method differs however, in the very essential respect of the use of a telephone receiver as the recording device, thereby making it possible to observe currents of high frequencies.[13]

A preliminary report of this investigation has appeared

[13] Our use of the telephone receiver was suggested by Wedensky's introduction of it in his classical experiments on inhibition; see N. Wedensky, De l'interference des excitations dans le nerf, *C. r. Acad. sci.*, 1893, 117, pp. 240 *f.*

recently.[14] A correspondence is shown between the frequency of sound stimulating the ear and the frequency of currents in the auditory nerve not only for very low sound frequencies, as the work of Forbes, Miller, and O'Connor has suggested, but for all frequencies at which observations have been made. Observations to date extend over more than five octaves, specifically, from 125 to 4500 ~/sec.[15]

The transmission process is one of great fidelity. A tone sounded into the cat's ear is represented in the nerve response so that the effect as heard in the receiver is indistinguishable in pitch from the stimulus tone as heard directly. Complex sounds, including speech, are communicated readily.

The correlation of frequency in the nerve response with frequency of stimulation is at variance with expectations as based upon the study of response in other sensory nerves, where, as stated above, response frequency has in every case been found correlated with stimulus intensity. The correlation found, together with the demonstration of high frequency responses in the nerve, makes necessary an examination of current theories of audition, and a reformulation of the possibilities that remain.

A Re-examination of Theories

In the consideration of present-day theories of audition, we may rule out first of all any such theory as that of Wrightson, where each cycle of pressure-variation in the stimulus is supposed to give rise to multiple responses in the nerve.[16] Wrightson's theory assumes nerve stimulation at maximum, minimum, and 'crossing points' of the wave at the cochlea; this would give a frequency transformation of 1 : 4, and in

[14] E. G. Wever and C. W. Bray, Action currents in the auditory nerve in response to acoustical stimulation, *Proc. Nat. Acad. Sci.*, 1930, 16, 344–350. An early announcement appeared in *Science*, 1930, 71, 215.

[15] There is no indication that 4500 ~/sec. represents the maximum frequency in the nerve, and the experiment will probably reveal higher frequencies by the use of increased amplification. The cat is known to react to tones in the neighborhood of 10,000 to 20,000 ~/sec.; see E. G. Wever, The upper limit of hearing in the cat, *J. Comp. Psychol.*, 1930, 10, 221–233.

[16] T. Wrightson, An enquiry into the analytical mechanism of the internal ear, 1918, Ch. 3, esp. pp. 121 *ff.*

our experiment we should hear at the receiver the double-octave of the stimulating tone. Since, as described above, the stimulating frequency itself is represented in the received signal, this type of theory cannot be maintained.

The evidence also, it seems to us, definitely disposes of the Helmholtz theory as it is ordinarily formulated today. As the above outline of the theory makes clear, the modern formulation posits a correlation of nerve impulse frequency with sound intensity, and no constant relationship between impulse frequency and sound frequency. Yet the latter relationship has been found to exist.

The variation of the theory proposed by Forbes, Miller, and O'Connor is also incompatible with our results. This theory assumes a correlation of stimulus frequency and response frequency only at the lower end of the scale, and certainly no higher than about 2000 per second; and in any case the straggling character of the repetitive discharge in the individual fibers would give no such response as demonstrated in our experiment, but would produce at low frequencies a very noisy tone, and at high frequencies probably mere noise with no tonality at all.

The Remaining Possibilities

We are now concerned with the remaining possibilities for auditory theory. At the present moment we see but four. These four represent the possible combinations of two alternatives, which are the acceptance or rejection of the resonance hypothesis, and the choice of either the telephone principle or what we call the volley principle. We shall discuss, therefore, the following: (1) a resonance telephone theory, (2) a non-resonance telephone theory, (3) a resonance volley theory, and (4) a non-resonance volley theory.

(1) *The Resonance Telephone Theory.*—It is perfectly possible to combine the resonance, peripheral analysis hypothesis with the telephone principle of frequency transmission. Sound frequencies are then spread along the linear extent of the basilar membrane, and particular frequencies involve particular nerve fibers; but when a nerve fiber is stimulated

it responds at the frequency of the stimulus. The theory thus avoids the postulation of a special stimulating process interposed between basilar membrane and nerve fiber to transform the frequency, as must enter with any but a telephone theory. Movements of the basilar membrane may be thought of as stimulating the connected nerve fibers directly through the bending of the hairs of the Corti cells, say, at the maximum of the wave as the hairs bear upon the tectorium.

To explain intensity this theory must appeal, as any telephone theory must do, to number of fibers stimulated. Greater amplitude of vibration of the basilar membrane may bring in new nerve fibers by two methods, through an increased linear spread of excitation or by the excitation of additional fibers in the region already involved. Either of these methods involves difficulties arising from the limited number of nerve fibers available, some 14,000 in human auditory nerve.

Assumption of a certain amount of spread is imperative. It is inevitable in any resonance theory, as Helmholtz clearly stated but most later discussions have ignored, because resonators partially damped respond in greater or less degree to a whole band of frequencies and not merely to that frequency to which they are tuned. It is unavoidable in a theory of the type in question because a sufficient number of fibers must be available for each pitch in order to account for its distinguishable loudnesses.

We have in this theory a further difficulty of the sort that in the Helmholtz resonance-place theory was solved by Gray's principle of maximum stimulation. Here, however, Gray's principle encounters a particular obstacle; if frequency in the nerve fibers follows pitch, it cannot at the same time indicate the point of maximum stimulation. We must resort instead to number of fibers per unit area of basilar membrane, assuming that the fibers vary in their stimulability, or else abandon this principle and put in its stead some such concept as that of median stimulation.[17]

This theory, like the old telephone theory, runs counter

[17] See, however, Hartridge's denial of the problem, in Foundations of experimental psychology, 1929, p. 343.

to the assumption that the intensity-frequency correlation demonstrated in other sensory nerves holds also in audition; and it assumes also a refractory phase of nerve low enough to pass the highest distinguishable tones. The theory has the particular merit of explaining Ohm's law and at the same time accounting for the binaural localization of tones in terms of phase difference.

(2) *The Non-resonance Telephone Theory.*—This is the familiar telephone theory, of the type outlined by Boring and discussed above. It accounts for pitch by frequency of nerve fiber response, and for intensity by spread of excitation and hence number of fibers responding. It accounts for binaural phase localization, but relegates analysis to the cortex. It takes the same position regarding Adrian's intensity-frequency principle and refractory phase as does the theory just outlined.

(3) *The Resonance Volley Theory.*—It is possible to formulate a theory which will include the chief features of the resonance telephone theory and at the same time fall into line with the Adrian intensity-frequency principle and the usual assumption that refractory phase in the auditory nerve is of the same order as that shown in other sensory nerves. The formulation involves what we call the volley principle, which may be stated as follows:

A given fiber when stimulated, let us say, by the bending of the hair of its Corti cell, responds, and then remains inactive for a time until it is again stimulable. The responses succeeding the first will fall somewhere in the relative refractory period; just where depends upon the intensity of stimulation. The fiber will thus respond to a constantly maintained stimulus with a regular series of impulses of lower frequency than the stimulus but in synchronism with the individual waves of the stimulus.

Any tonal stimulation will involve a number of fibers, each responding as suggested, but if we can assume these fibers to vary somewhat in their refractory properties, their rates of response will vary. Though the fibers may be out of synchronism with one another, they are all synchronized with

the exciting frequency, and the result of their combined response is a representation of the exciting frequency in the nerve as a whole. The situation is something like beating a tattoo with the two hands working alternately, and establishing a total frequency double that of either hand. Figure 2 is a representation of the theory for a few fibers. All the

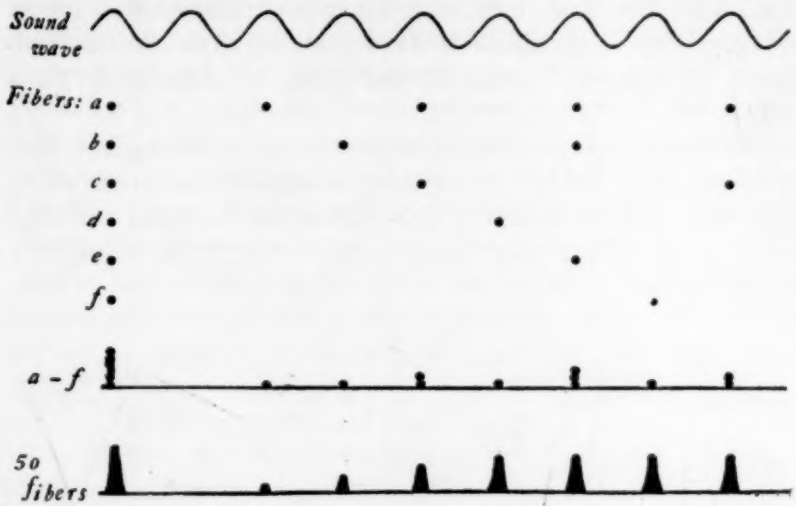

FIG. 2. The Volley Theory

Each fiber responds at a frequency below that of the sound waves, but in strict synchronism with the sound waves. A number of fibers might give some such effect as shown at the bottom of the figure, representing faithfully the frequency of the sound.

fibers, as shown, respond at the first wave, but fiber *a*, having the shortest refractory period, responds again at the 3d wave, later at the 5th, and so on. Fiber *b*, with a little longer refractory period, comes into the volley at the 1st, 4th, 7th, etc. And so with the other fibers, their rate of entering the volley being dependent, for a given intensity, upon their rate of recovery in the refractory period. For the sake of simplicity the rate of response of the individual fibers is represented as constant, which means that the refractory periods do not vary. But this is not necessary to the theory; a change in the refractory periods of the fibers during stimulation would not interfere with the synchronous character of

the response, but would merely alter the total number of impulses in the response.[18]

It may be noted that on this theory several vibrations are necessary to determine the perception of pitch, as several different fibers must enter the volley before the frequency is represented. Moreover, the higher the frequency the greater the number of vibrations for pitch perception; and all this is in agreement with the little data that we have on the minimum number of vibrations necessary to determine pitch perception.[19]

As has been intimated, with greater intensity a given fiber will fire more rapidly: it may, for example, come in at every 4th wave instead of every 5th. Thus in the total response

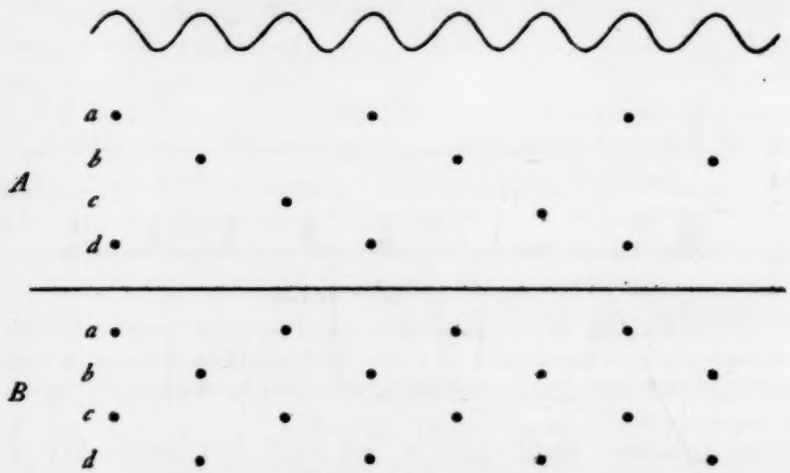

FIG. 3. Intensity according to the Volley Theory

A represents a tone at a given intensity, with four fibers, *a*, *b*, *c*, *d*, responding. *B* represents the same tone at a higher intensity, and the fibers responding at a higher rate. The exciting frequency is in each case represented, but in *B* with a greater total number of impulses.

there will be a greater absolute number of impulses, though the established frequency remains the same. Let the two parts of Fig. 3 represent schematically the response of a few

[18] The theory could agree with the findings of Field and Brücke, *Arch. f. d. ges. Physiol.*, 1926, 214, 103–111.

[19] R. Bode, Die Zeitschwellen für Stimmgabeltöne mittlerer und leiser Intensität, *Psychol. Stud.*, 1907, 2, 293–323.

fibers after a tone has been sounding for a short time, *A* representing the tone at a given intensity, and *B* the same tone at a higher intensity. It is plain that in the individual fibers, and hence in the nerve, Adrian's intensity-frequency relationship holds. And it is also evident that the refractory phase need be no different from that already demonstrated in other nerves.

This theory, like all resonance theories involving spread of response on the basilar membrane, will perhaps invoke some such principle as Gray's principle of maximum stimulation to account for the determination of pitch. Here the maximum determining the pitch can be accounted for very simply in terms of nerve fibers showing the maximum rate of discharge: those fibers corresponding to the region of the basilar membrane best tuned to the applied frequency will receive the most vigorous stimulation and hence, responding earlier in their relative refractory phase, will work at a higher rate.[20]

(4) *The Non-resonance Volley Theory.*—This theory differs from the last mentioned principally in the denial of resonance to the peripheral mechanism. The theory differs slightly, but not essentially, in its account of intensity; instead of accounting for intensity by greater spread over limited regions of the basilar membrane, as the previous theory does, it considers the spread over the membrane as a whole. Maximum loudness of a tone, then, would involve the whole membrane, and hence impulses from all fibers of the nerve. The theory is in accord with Adrian's intensity-frequency principle and assumes refractory phase of the order known in other nerves. It accounts for binaural phase localization, but leaves analysis to be taken care of centrally.

Many minor variations and developments of the above theories are possible, but cannot of course be taken account of in a brief outline. The theories differ somewhat, as is evident, in their capacity to explain various auditory phenomena, but so far as we can see, each of them is possible at

[20] This type of theory has been formulated by H. Fletcher, Speech and hearing, 1929, pp. 111 *ff.*, 191 *ff.*

the present status of our knowledge. We refrain at this time from expressing a preference for any one of these theories, and wish rather to look to further investigation for a decision. This discussion is made, not to propose or to defend a theory, but in the attempt to analyse the problem of audition in the light of the evidence now available, and thus perhaps to converge further developments in the direction of a final solution.

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PSYCHOLOGY AND THE PSYCHOGALVANIC REFLEX

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The fascination which the psychogalvanic reflex has held for psychologists in general, is a remarkable thing. I think this attraction is based principally upon two things. The psychologist would like to believe that at last he had a method by which he might detect, and perhaps measure, that variety of human experience known as emotion. The second, and perhaps the greater lure, is that in measuring these changes he makes use of electrical instruments which at least give a semblance of physical exactness to his research. The psychologist has suffered from a variety of inferiority complex because his instruments were not as beautifully exact and impressive as those of most of the other scientists, hence the use of galvanometers and Wheatstone bridges afforded a means of sublimation. The result of all this is shown in the fact that there have appeared during the past forty years some 325 titles which bear in some way on the problems of the electrical properties of the skin. About seventy-five of these consider the psychological significance of the phenomena. Almost every article which considers this psychological significance is controversial in the sense that positive or negative statements concerning its meaning are contradicted by one or several other investigators. The result is that today the psychological meaning and importance of the problem is a point of serious dispute and doubt.

It has always seemed to me that many debatable points in science are more understandable and the truth more easily arrived at when one is in possession of a fairly accurate historical perspective. There are several points of historical importance which color most modern thinking on this subject. In brief, it is commonly held that this reflex is a criterion of

some psychological function or category and this category is usually termed 'emotion.' The history of the experimental work on the psychogalvanic reflex offers an interesting example of the way in which concepts are uncritically set up and conclusions misinterpreted before finding a place in present day text-book psychology.

In a recent review of the literature (8) I cited Vigouroux's statement of 1888 that the problem of alterations of the resistance of the human body were investigated by Bertholon as early as 1786. Both the French and German scientists in the first half of the 19th century were interested in the problem of animal electricity and animal magnetism. (The early confusion of these concepts is interesting of itself.) Psychological interest in the problems of the significance of the alterations in the electrical properties of the human body may be said to start with the publications of Vigouroux in 1879. Vigouroux had been associated with Puyfontaine who had investigated the electrical signs of voluntary action and of animal magnetism. Vigouroux turned his attention to electrical measurements as they might be of diagnostic value for the medical clinic. In 1879 he published his first paper entitled 'Sur le rôle de la résistance électrique des tissus dans l'électrodiagnostic.' He reported the same year another paper of the same title to the Société de Biologie. His work caught but little attention, and that was limited to those interested in electro-therapeutics.

On January 14, 1888, Féré reported a study of the modifications of the electrical tension of the human body to the Société de Biologie. In this paper Féré gives voice to the belief that the static charge of electricity carried on the surface of the body, is influenced not only by weather, state of nutrition of the skin, and such physical factors, but also by various psychological events, such as sensorial stimulation, emotional experiences and the influence of the moral emotions. This paper of Féré's was sharply criticised by d'Arsonval and by Vigouroux at the meeting of the same group on February 11, 1888. D'Arsonval pointed out at this time that potentials such as Féré described could in no sense have an organic

origin but could only be due to the friction between the skin and the clothing.

On March 3, 1888, Féré replied with a report of an investigation of the electrical resistance of the body as influenced by sensorial excitation and by emotion. (In this paper he ignores the claims which he made in his earlier paper as well as the remarks of his critics.) In 1892 Féré published a 'Pathology of Emotions' (4) where he reiterates and amplifies his original contention that the electrical tensions of the human body are influenced by sensorial or emotional excitation. Since this book has not been noted in the psychogalvanic literature hitherto, I shall quote from the English translation at some length. He says in part:

> The electric tension and the intensity of the electrical discharges increase further under the influence of the moral emotions. One of the first facts which have been remarked is that the crepitation becomes increased in the wake of audition of certain bits of music which induce a great general excitation. The cracklings manifest themselves over all the body spontaneously without the approach of any foreign body, and determine mainly in the limbs a very disagreeable sensation of prickling. Further, a large number of peripheral excitations provoke the same increase of tension, a noise, a smell, a change of light, etc. I have verified several times modifications of the lie of the hair under influence of this order.
>
> Dry seasons favor these electrical phenomena, which are especially marked during frosts; wet and foggy seasons produce opposite effects. The modifications of electrical tension, which is nil in times of rain or south wind, precedes, sometimes several days in advance, a change of weather.
>
> To the exaggeration of tension corresponds a state of general excitation; a very exactly appreciable hyper-activity. When, on the contrary, under the influence of atmospheric humidity, tension diminishes, there is produced a sensation of lassitude, powerlessness.
>
> Under the influence of excitations of the periphery there is produced with Mme. X., a deviation to the right (125 volts) when one has made her look through a blue glass, and much more considerable when one has brought a bottle of ether near her. (Quoted from pages 164, 165, and 166 of the English translation.)

Most psychologists have attributed to Féré the notion that the electrical charges of the body are of psychological

significance. In view of the above sample of Féré's psychological insight and acumen it seems that it might be well to investigate further the history of the notion.

The second line in the development of the subject was that started by Tarchanoff by his experiments reported in 1890. The work of this Russian physiologist probably found its original suggestion in that of duBois-Reymond, although I have been unable to find any direct proof of this. Tarchanoff made a very thoroughgoing investigation of the variations in the electrical output of the human body as they are shown when the body is connected directly with a sensitive galvanometer. He believed that these variations were due to variations in the activity of the sweat glands and showed that they might be caused by a great variety of stimuli. He was able to evoke this reflex by the ringing of a bell, the smelling of various odors, tasting acid or sugar, by having the subject think of joy, or by being expectant, by attention and by voluntary movements. At no point does he say that the phenomenon in any way pertains exclusively to emotion. His experiments drew the attention of Sticker (1897) and of Sommer (1902). These investigators were interested in the physical and physiological basis more than in the concomitant psychological activities. In 1903 Waller reviewed Tarchanoff's experiment and remarked, "I do not think these observations on the human subject are particularly satisfactory." (This observation is particularly interesting when one remembers that between 1918 and 1922 Waller published a series of fifteen articles on this general subject, none of which were as satisfactorily controlled as Tarchanoff's experiment and all of which put forth the contention that the phenomenon was an indicator of true emotion or affective expression.) Despite the work of the French school and of Tarchanoff and his followers, this phenomenon gained no particular scientific attention or interest.

In 1904 E. K. Müller, an electrical engineer of Zürich, rediscovered these galvanic alterations of the skin and called them to the attention of Veraguth, a psychiatrist and neurologist. Veraguth was evidently very interested in Müller's

experiments and undertook a series of experiments on the subject which were finally printed in his book called 'Das Psycho-galvanische Reflex-Phänomen,' published in 1909. Veraguth's work need not be gone into in detail. Certain points, however, deserve comment. He was confident that the phenomenon had its origin in changes of activity of the sweat glands and that it was in no sense a vaso-motor occurrence. He held that not every stimulus would cause a reflex; only those which were sensed and real were found to be effective. Those stimuli which were accompanied by a psychological compulsion which was marked by stress or acceleration, gave rise to the largest galvanic changes. Thus he considered that galvanic change is an indication of stress and actuality of psychological events. In another paper he pointed out that the reflex is a good method for investigation of emotional experiences, since it gives a mode of distinction between actual and senile complexes. From this last statement it is clear that Veraguth was thinking in the terms of Jung and the Zürich psychoanalytical school.

In 1907 Peterson and Jung published their first study of the use of the galvanometer with normal and insane individuals. During the next three years Peterson and Jung published several additional studies on this subject pointing out that the galvanic changes were good measures or indicators of emotion. Indeed Peterson went so far as to hold that it is true that the galvanometer is a measure of emotional tone and of emotional conditions in the unconscious sphere. I think that the argument of Jung and his associates might be fairly stated as follows: (*a*) A complex is a system of emotionally toned ideas which have been repressed into the unconscious. (*b*) When in an association test abnormal responses or prolonged reaction times are elicited, such reactions are found to be connected with complexes. (*c*) The psychogalvanic reflex almost invariably appears together with these abnormal reactions. (*d*) Hence (on the basis of *a*, *b* and *c*), the galvanic reflex is exclusively associated with emotion. Figure 1 illustrates the obvious inadequacy of the logic on which statement (*d*) is based. It is only too apparent

that this relationship entirely neglects the cases where the reflex appears without being in any way associated with a complex, as in sleep, in acts of volition, in decerebrate animal preparations, etc. As Fig. 1 shows, 'emotion' and 'psycho-

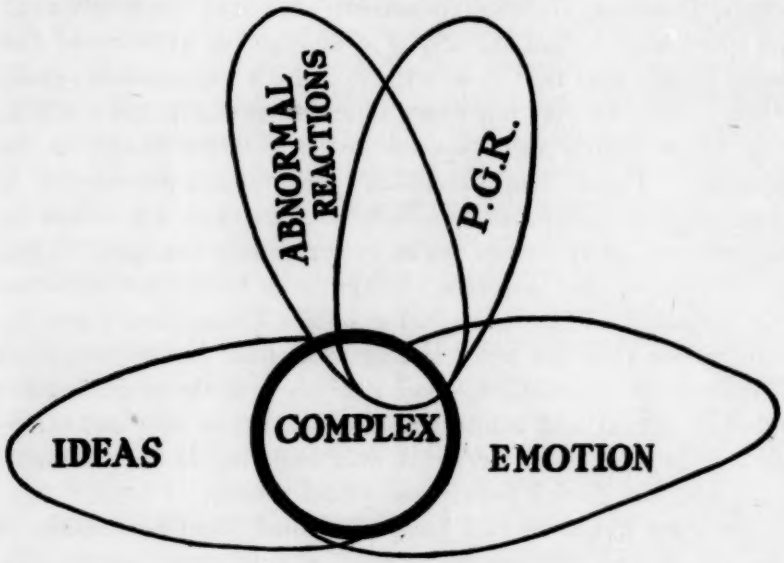

FIG. I

galvanic reflex' are not mutually inclusive even in the terms of psychoanalytic logic. It should also be noted that (*a*) is a definition which must itself be experimentally proven. In view of the fact that the theory of the close alliance between the galvanic reflex and emotion is to be traced directly to the uncritical agreement of subsequent investigators with Jung's conclusions, it is clear, on the basis of the analysis which I have just made that the theory was not originally adequately demonstrated and should have been questioned years ago.

F. L. Wells in 1908 published a critique of the psycho-galvanic reflex experiments and found that it was precarious to accept the galvanometric evidence of emotion without introspective confirmation. This careful and critical work of Wells was overshadowed in popular interest by the work of Sidis, who held, on grounds which Dunlap showed were inadequate, that the reflex was under the influence of the

affective states and essentially an indicator of emotion. Sidis' influence was reinforced by that of Piéron, who, beginning in 1910, expressed dogmatically the view that the galvanic reflex was specific to emotion and that there was 'only one factor capable of provoking it—the affective factor.' Piéron's early notions concerning these phenomena were evidently based on insufficient evidence, since during the course of three succeeding years he withdrew or reinterpreted many of the statements which he made in his original paper, excepting only that of the doctrine of emotional specificity.

From this résumé it is clear that the earlier workers merely pointed out that affective experiences along with many other events might evoke the reflex. Féré, who believed that moral emotions caused sparks to emerge from the body; Jung, who held that the vitality of a complex was measured by the galvanometer; or Veraguth, who said that the reflex was an indication of psychic stress, all illustrate the point that scientifically as well as psychologically no careful reader is justified in saying that the work of these investigators demonstrated that the psychogalvanic reflex is connected in any special fashion with emotion, as the term emotion is understood in modern psychology.

Now let us turn our attention for a moment to modern psychology, at least in so far as it is exemplified in our current psychological treatises or texts.

Ladd and Woodworth (7) include a brief survey of the literature on the subject and state, "Though these electric phenomena are delicate indicators of emotional states, they do not apparently differ with the particular quality of the emotion." Wallin (13) gives a summary of the clinical applications which have been made of the phenomenon, showing that it is most frequently associated with emotional processes but that there is evidence to show that it may be connected with conation. Poffenberger (11) mentions the use of the reflex as an indicator of an emotion or excitement. Morgan (9) states, "This method may be used to detect whether a person responds emotionally when there is good reason to believe that he is restraining the expression of his

feelings." Dashiell (3) states, "It is assumed that the amount of increase . . . in the excursion of the galvanometer . . . measures the amount of emotional excitement aroused." Herrick (5) states, "The psychogalvanic reflex has been used to find out whether the body is reacting vigorously or not to strong emotion." Woodworth (15) says, "This electrical change is perhaps the most delicate indicator of emotion that the laboratory has yet produced." Brooks (2) says, "This increased deflection of the needle of a galvanometer upon the presentation of a given stimulus is regarded as an indication of its having possessed an affective tone." Finally and most remarkable of all, Perrin and Klein (10) state, "Nerve impulses are known to generate electricity, hence when the galvanometer is connected by wires with the human body it presumably responds to neural activity. It has been used successfully, however, only in the case of an emotional response."

It seems from this sample of text-book statements that there is rather a general text-book agreement that the reflex is a valid criterion of emotion and even possibly a measure of the amount of emotion, though perhaps not a means of differentiating emotions. Does the more recent experimental literature of physiology and anatomy as well as that of psychology confirm this view?

In 1922 Schilff and Schuberth published in *Pflüger's Archiv* a study of the centripetal and centrifugal anatomical pathways of the electrical skin responses (the so-called psychogalvanic reflexes) in the frog. Their work shows quite clearly that the response depends on the integrity of the sympathetic division of the autonomic nervous system and the connection of the division with certain centers in the medulla and mid-brain. Foa and Perserico in 1923 repeated these experiments by sectioning appropriate nerves or the spinal cord of cats, checking the results by the action of drugs. They localized the center of the galvanic response in corpora quadrigemina and calamus scriptorius. The centripetal pathway follows any sensory nerve tract reaching the central nervous system. Centrifugally the pathway descends

from the bulbar-pontine region through the spinal cord. The impulses leave the cord and travel over the sympathetic motor fibers in the major motor nerve trunks to the skin, connecting with the sympathetic plexuses of the skin.

When one investigates the physiological publications on the subject of the electrical phenomena of the skin the names of Gildemeister, of Strohl, and of Philippson stand out as representatives of the thorough, sound, and careful investigators who have given both time and thought to this problem. Gildemeister has published at least eighteen papers which bear directly or indirectly on this subject. It is due to his work that we today have some understanding of the real physiological and anatomical basis of that phenomenon which we call the psychogalvanic reflex. Gildemeister approached the problem from the standpoint of the physiologist and has been interested in it as it shows changes in the permeability of the cell membranes. He has demonstrated that the changes in resistance which are shown by the galvanometer are dependent for the most part upon changes in the membranes of the sweat glands. These changes are under the control of the pilo-motor fibers of the autonomic nervous system. As long ago as 1918 he stated that the phenomenon should be called either a galvanic skin reflex or a general autonomic reflex. He has shown that the phenomenon is a constant physiological factor which is varied in its physical components only by changes in temperature. A feature which Gildemeister finds intriguing is the fact that somehow the permeability of the cell membrane may be changed by the influence of the nervous impulses. This, of course, is a point of major importance for the physiologist.

Strohl has published some seventeen papers which report investigations of the physical and physiological basis of the electrical phenomena of the skin. His work on resistance, conductivity, rate and degree of polarization offer us a picture of what complete, thorough research may be. For instance, he has shown (**12**) that a counter electromotive force of 16.2 volts is set up in .00005 second after the application of 20 volts to the skin. In .0001 second that has increased

to 17 volts, in .005 second to 17.3 volts and in .01 second has fallen to 16.8 volts. Since the apparent resistance of the body is a function of this counter electromotive force plus true resistance, this illustrates an essential weakness in psychological research which depends upon differentiating individuals on the basis of resistance. Of course the factors of thickness of skin, place of attachment of electrodes, moisture of the skin, temperature of the skin and vascularity of the skin are equally important to these counter electromotive forces. Strohl has gone so far as to arrange a schematic model which reproduces the physical conditions of electrical excitation in man. That is to say, he has by means of a mechanical and electrical device found it possible to reproduce the electrical variations shown by the skin. Philippson and Menzerath have shown that the reflex is a complex manifestation in which variations in vascularity, in the muscular tonus, and in the secretory mechanisms of the skin all play a role.

This reflex, as Gildemeister pointed out in 1922, is a part of the general autonomic response which is produced whenever stimuli are applied to the human body. He notes that further objectively observable concomitants of this reflex are the pupillary response, various vaso-motor changes, changes in cardiac rate, contraction of the muscles of the hair follicles and changes in skin temperature. To his list I would add respiration, muscular activity and changes in pH of blood and saliva. We have, then, an electrical response which may show itself either in a change in apparent resistance or in apparent electrical output or in polarization which appears as a concomitant of, or a result of, any of the factors which we have just listed. And as Gildemeister has stated, all of these factors are under the general control of the autonomic nervous system. To paraphrase Gildemeister, one may state with confidence that *no law has as yet been found which governs these fluctuations and changes or governs the strength of the reflex.*

This brief survey of certain of the anatomical and physiological findings reported in the literature shows, I believe, that although many points are obscure, still the main outlines

which explain these phenomena have been very beautifully worked out and are open for the guidance of any one who seriously considers the investigation of these problems. If we turn for a moment to the work of the psychologist it is of more than passing interest to note that there is a wide variety of opinion as to the meaning of the reflex in psychology. I find in going through the literature that the psychogalvanic reflex has been elicited by the following varieties of stimuli or stimulating conditions; sensations and perceptions of any sense modality (sight, sounds, taste, etc.), associations (words, thoughts, etc.), mental work or effort, attentive movements or attitudes, imagination and ideas, tickling, painful or nocive stimuli, variations in respiratory movements or rate, suggestion and hypnosis, emotional behavior (fighting, crying, etc.) relating dreams, college examinations, and so forth. In an attempt to classify statements which various investigators have made concerning the psychological significance or the psychological concomitants of this reflex, I have formulated the following résumé: Forty investigators hold that it is specific to or a measure of emotion or the affective qualities; ten others state that it is not necessarily of an emotional or affective nature; twelve men hold that it is somehow to be identified with conation, volition, or attention, while five hold very definitely that it is non-voluntary; twenty-one authorities state that it goes with one or another of the higher mental processes; eight state that it is the concomitant of all sensation or perception; five have called it an indicator of conflict and suppression; while still four others have used it as an index of character, personality or temperament. To list all the statements on which this tabulation is based would add but little to the discussion. The essential fact remains that either the psychologist does not know the characteristics of mental activities or he is unable to agree with other psychologists as to the name to be given to these activities when they do appear.

The question of the psychological correlate of the reflex has been studied in some detail in recent years by Wechsler, Bartlett and Aveling and his associates. The work of

Wechsler (14) is frequently cited as offering conclusive evidence that the reflex is specific to emotion and, to a degree, a measure of emotion. Wechsler based the most important part of his conclusions for our purposes on the introspective reports of 10 young women students at the New York School of Social Work who were given an association test of 26 words and asked after the experiment to assign on a five-fold scale the emotional value which each word originally inspired. The correlation coefficient obtained between the amount of deflection and the assigned emotional value was found to be .59 and .67 for two different groupings of the words. From these figures one may surmise that although there was a tendency for agreement, many negative instances occur. Certain of these Wechsler discusses and then concludes: "I think the above are sufficient to allow the inference that the galvanic reflex may be an index of unconscious affective tone." It seems to me that Wechsler's demonstration does violence to the rules of scientific logic. As I see it, he says in effect: 'Practically every form of higher mental process carries as a component of itself emotion or affective qualities. When the reflex appears and the subject is unable to identify the emotion it is evidence of unconscious emotion, since I have assumed that the reflex and emotion always appear together.' Wechsler himself elicited the reflex by applying painful stimuli to sleeping subjects. Likewise he agrees that the reflex appears as a result of violent respiration, grasping or sighing. In both of these cases there is no reason to suppose that emotion is a necessary or even an expected part of the reaction. What it all boils down to is that Wechsler should define *any* bodily activity or reaction appearing with the reflex as emotional. If, then, other psychologists would agree to this fundamental definition Wechsler might proceed with his argument in the fashion which he has already followed. Since he did not state this definition and since many psychologists would probably disagree with it if he did, I can only conclude that his logical case fails as completely as did his attempted empirical demonstration.

Aveling's argument for the conative basis of the phe-

nomenon has been well summarized as follows by Bartlett (1).

1. *Emotion* (awareness of somatic resonance) is not the cause, because it is 'most frequently subsequent to the experience related in the relevant introspections.'

2. *Affection* (pleasure-unpleasure) is not the invariable cause because it is seldom reported when there are deflections and when reported is often unaccompanied by deflection.

3. *Conation* is *a* cause, possibly *the* cause, because subjective estimates of conative effort (alertness and striving) tally with the size of deflection recorded.

Bartlett carried through a rather well-planned experiment (he omits any account of the physical technique) in an attempt to ascertain more exactly, if possible, the psychological component or components accompanying the reflex. He concludes, "The psychogalvanic phenomenon follows a variety of complex changing mental states many of which would ordinarily be described as 'emotional' and yet may yield to introspective enquiry cognition and/or conation only. It seems best in our present state of knowledge to describe these complex states as 'orectic process' in which 'feeling' and 'striving' are mingled . . . it seems probable that the mental cause of the physiological change that brings about the deflection is of the nature of passive endurance or enjoyment rather than of active striving or willing." [1]

Allowing this to stand for a moment I wish to cite a different sequence of evidence which bears on the crucial point. No one, so far as I am able to find, has yet reported instances of the occurrence of the reflex in situations in which behavior that would be classed by any observer as 'emotional' occurred. That is to say, all reports so far deal with mild emotion or affection of the more or less artificial laboratory variety.

[1] That these conclusions of Bartlett are not so final as he believed is shown by the more recent papers of R. B. Cattell (*Brit. J. Psychol.*, 1929, 19, 357–386) and of T. M. Abel (J. Exper. Psychol., 1930, 13, 47–60). Cattell holds that Bartlett's hypothesis that the reflex is connected with 'passive enduring' is contrary to the facts, but that the magnitude of the reflex is highly correlated with 'force of character,' and closely connected with conation. Abel finds that the reflex is not correlated with any psychological class, as emotion, volition, or conation but that it is related to sudden checks in the course of comprehension.

In a recent experiment with delinquent boys [2] I was able to secure records of galvanic changes while the boys were working continuously on a pursuitmeter. The average frequency of appearance of the reflex for the entire group of 100 boys which I used was 32 times during the forty minute period of the experimentation. Five subjects were decidedly nervous, jumpy, or apprehensive and in those cases the average frequency was 40. Five boys were frightened during the experiment and showed an average frequency of 7.6; four boys were quite angry and showed an average frequency of 16.7; three boys who cried showed a frequency of 8.6. It is to be noted then that the reflex appears more frequently than one would expect in individuals whose behavior indicated apprehension but in all other cases of clearly marked emotional behavior the reflex appears only one-half or one-quarter as frequently as one would expect.

Summary

In any attempt to evaluate the psychogalvanic reflex as a device of psychological importance certain empirical facts must be kept in mind. (1) It has been demonstrated that the reflex is under the control of the sympathetic division of the autonomic nervous system. The center of this control is found to be localized in or near the calamus scriptorius. Kuntz (6) gives evidence which indicates that the fibres of the sympathetic nervous system running to the sweat glands of the skin are not overly specific in their action, nor do they necessarily follow the usual laws governing other types of sympathetic nervous activity. (2) The reflex is associated with the secretion of sweat, with vaso-motor changes, changes in the pilo-motor musculature, and probably with changes in the pH of the blood or lymph of the skin. (3) The reflex may, and frequently does appear as a concomitant to any one of the above list of physiological changes. That is to say, it is not necessary for all of these physiological changes to occur in order that the galvanic reflex should be elicited.

[2] Manuscript entitled 'An attempt to measure emotional traits in juvenile delinquency' to be published soon.

(4) As Gildemeister points out, no law has been found which governs these fluctuations and changes or governs the strength of the reflex.

Added to these are certain points of psychological and logical importance which should be considered. (1) A survey of the literature on the subject shows that these electrical phenomena have been considered by uncritically minded investigators to have necessarily a certain psychological importance. Either implicitly or explicitly stated, the argument may be put as follows: 'The reflex appears together with various psychical or mental events; hence it must have a mental correlate or a controlling mental function.' As a matter of fact this argument has thus far been demonstrated only by the method of proclamation. (2) A frequent method of 'testing' whether or not the reflex might be associated with one or another of our poetical psychological categories has been to arrange an experiment in which the conditions emphasized the required category and to observe the correlation between the appearance of the reflex and the category in question. In this way it has been demonstrated as correlating in a more or less satisfactory fashion with conation, volition, attention, ideation, mental stress, introversion-extroversion, mood, dreams, intelligence, memory, association, etc. Such experiments have only unnecessarily encumbered the literature without adding to our real systematic knowledge of the subject.

If these points are true, just what is the place and use of the psychogalvanic reflex in psychology? It seems to me that our knowledge concerning the psychological significance of this reflex might be summed up, somewhat as follows: (*a*) The appearance of electrical changes of the skin can be taken as an indication of change in the balance of physiological functions of the skin brought about by nervous impulses following pathways in the autonomic nervous system. In this sense the reflex is to be compared in psychological significance to changes in the basal metabolic rate of the body. The metabolic rate is the final product of both anabolic and catabolic changes. In the same way the reflex represents an

end product resulting from changes of the cell-membranes of the sweat glands, vaso-motor variations, temperature changes, etc., which have been mediated over to the autonomic nervous system. (*b*) The appearance of this galvanic change has the same psychological significance that a change of blood pressure may be said to possess. It is well known that augmented stimulation (either physical or mental) brings about changes in blood pressure. It is also known that changes in blood pressure take place as the result of physiological changes within the body, which changes have no known psychological bearing, correlation, or significance. The same may be said of the psychogalvanic reflex. (*c*) There is no conclusive evidence which would indicate that the psychogalvanic reflex is a measure of, regular criterion of, or indicator of, any one or combination of the traditional psychological categories. Despite textbook statements, no one has yet demonstrated that there is any essential relationship existing between emotion and the reflex. Aveling and his associates use the term 'conation' in so wide a sense that it may to all intents and purposes be considered as an element of practically any 'mental or psychical' reaction. There is fairly good evidence to show that the reflex *may* go with any variety of 'mental' or 'conscious' reaction. The evidence is equally good in showing that the relationship is not invariable. The reflex may be elicited without demonstrable 'conscious' concomitants, as in sleep or in decerebrate animal preparations. From all this I can only conclude that the psychogalvanic reflex has about as much significance as an indicator of emotion as has the pupilary reflex, the knee-jerk, or the vaso-motor reflexes. Further, its general psychological significance is comparable on equal terms to these same reflexes.

This much vaunted expressive reaction finally shows itself as having only a very limited psychological meaning. I feel that this loss not only demonstrates that another 'test' has failed, but that it points to the very real necessity for psychologists to clarify their thinking with respect to the psychological categories employed in psycho-physiological correlation. We all of us know that every attempt which has been

made thus far to find physical or physiological correlations for psychological categories has failed, and failed with an all too thorough-going completeness. Blood pressure as an indicator of mental activity, blood volume as a method of demonstrating feelings, changes in metabolism as a method of demonstrating mental efficiency, visceral reactions as a basis for emotion, changes in resistance as measures of the depth of sleep, and now the psychogalvanic reflex as a measure or indicator of emotion, to mention only a few of the many which have failed to give the much hoped for psychophysical correlation. There is something seriously wrong with our psychological notions. The psychological categories need either a thorough overhauling or to be scrapped. I should like to ask if there is any reason why psychologists need to cling to such poetical terms as emotion, volition, ideation, attention, etc. These terms, although descriptive, defy definition and confirmation by experimental procedures. I am aware of the work of certain of the Gestalt psychologists which would indicate perhaps that much psychological experience is beyond analysis. This notion is intriguing, but just at present it seems to me that it offers no satisfactory solution to the problems which I have discussed here.

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OBSERVATIONS ON THE NATURE AND MECHANISM OF SECRETORY INHIBITION [1]

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In the various studies that have been made of conditioned salivary responses, little consideration has been given to the extent and complexity of the reaction itself. Pavlov, in whose laboratory most of the work on conditioning has been done, was not interested in this phase of the problem. "In this research we were not concerned with the ultimate nature of excitation and inhibition as such" (4, p. 377). To the psychologist, however, the functioning of the efferent side of the behavior unit is quite as important as the central or afferent aspect. In fact, a complete understanding of the latter must await a fuller description of the former. Undoubtedly misunderstandings of the nature of conditioning have arisen because of misconceptions of the extent of the response involved. It is our purpose in this paper to present data by which some idea of the complexity of the conditioned response may be obtained and to consider the major concepts of secretory inhibition in the light of these facts. No consideration will be given to qualitative variations in the secretions although it is recognized that this is a very important aspect of the reaction. What is said about inhibition will be intended to apply only to inhibition of nutritive functions and not to other types of inhibitory processes.

The popular impression that the conditioned salivary reflex is definitely limited to the parotid gland is largely the result of inference rather than evidence. The fact that a limited and localized aspect of the total response has been used as a basis for determining variations in the strength of the

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response may have been misleading. Although he does not discuss this problem directly, Pavlov seems to recognize the point that the salivary response is but a part of a more complex reaction. "This activity of the salivary gland cannot be regarded as anything else than a component of the alimentary reflex. Besides the secretory the motor component of the food reflex is also very apparent in experiments of this kind" (4, p. 22). It is not clear from this statement to what extent other secretions are involved in the reflex, but in a subsequent chapter he submits tables (4, p. 64) showing how the submaxillary secretions are affected simultaneously with the parotid secretions when the dog sees meat powder at a distance. There is evidence here that at least other glands of the oral cavity are involved in the conditioned response.

In our experiments with parotid secretions in humans, we have frequently attached suction discs to the Stenson duct on both sides of the mouth to observe any differences in quantity from the two sides under various situations. When, under this arrangement, we were able to evoke a natural conditioned response to the sight of food, it was noted that the increased secretion was bilateral. If there was a 200 percent increase on one side there would be approximately a 200% increase on the other. Distinction should be made here between the generalized nature of the conditioned response, shown above, and the localized nature of the unconditioned response. The direct response to oral stimulation, whether chemical or motor, appears to be definitely localized. Chewing food on one side of the mouth may increase the parotid secretion from that side several hundred percent more than that obtained from the opposite side. Tables showing this effect of direct stimulation were presented in an earlier report (6). It was this unilateral response to direct stimulation and bilateral response to indirect stimulation which aroused our curiosity and suggested the need for a better understanding of the conditioned response.

Evidence of the extension of the conditioned secretory response to the glands of the stomach has been obtained from physiological studies of digestive functions in animals and incidental observations of clinical cases in humans.

According to an account by Bogan (1) a boy three and a half years old drank some lye and gradually developed complete stegnosis of the æsophagus. He was taken to the children's clinic at the University of Heidelburg, and a gastric fistula was made and used for feeding, while curative measures were attempted. The following observations were made with this patient.

The child was laid on his stomach on two outstretched towels which did not meet, but allowed the canula to pass between. The patient could not see what was going on in the room. He was then fed milk or meat, and the gastric juices would begin flowing, although the food could not reach the stomach. After six such experiments had been made, it also happened that the sight of food called forth a secretion of the gastric juice. The child was then fed forty times while a certain tone of the trumpet was sounded. All of the experiments gave positive results. Finally, in ten trials with the trumpet seven gave a secretion. Anger or pain delayed the secretion.

The flow of gastric juice excited by food in the mouth may or may not have represented a conditioned response, but undoubtedly the secretion of these glands to the sight of food or to the trumpet indicates a conditioned response. Although no observations of the parotid secretion of this subject were reported, it is probably true that they also were registering an increased flow to the same stimulus which evoked gastric secretion. If so, we have here a significant indication of the spreading of the conditioned secretory response to the gastric secretions.

In his observations of a subject who had a gastric fistula of twenty years standing, Carlson also noted that "the mere act of chewing indifferent substances and the stimulation of nerve endings in the mouth by substances other than those directly related to food cause no secretion of gastric juice" (3, p. 235); while "the act of selecting the ingredients for the noonday meal caused a slight and temporary augmentation of the secretion rate of the empty stomach" (3, p. 238). Here again is an indication of the general nature of the con-

ditioned response and of the limited nature of the unconditioned response.

Evidence that digestive juices other than the gastric are involved in negative response is contained in a report reproduced by Cannon. "Recently Oechsler has reported that in such psychic disturbances as were shown by Bickel and Sasaki to be accompanied by a checking of the secretion of gastric juice, the secretion of pancreatic may be stopped and the flow of bile definitely diminished" (**2**, p. 11). These facts would seem to warrant the assumption that the conditioned salivary response is but a component of an extensive and complex reaction involving at least the other glands of the mouth and the glands of the central digestive tract.

In an effort to account for this complex response or to explain why these nutritive glands secrete positively under one set of conditions, while the secretion is blocked under other conditions, one turns first to an analysis of the neural mechanism involved. According to the physiological literature (**5**, p. 574) each one of the large salivary glands, like other visceral organs, receives nerve fibers from two sources: from the cerebrospinal and from the sympathetic system. Innervation of the sympathetic supply to the parotid gland causes no secretion to flow, but the influence of the excitation is shown by the occurrence of pronounced histological changes in the gland cells. Excitation of the cranial nerves going to these glands causes a secretion of saliva, the concentration and rate of which depends upon the strength of the stimulus. Pavlov has shown that the severance of these nerves prevents the development of positive conditioned responses. There appears, therefore, to be a very definite dual mechanism, providing for a positive response in one case and a negative or inhibitory response in the other. The sympathetic system which supplies inhibitory innervation is well arranged for the generalized type of response noted above. In discussing the organization of visceral nerves, Cannon makes the following statements relative to the extensive distribution of neurones of this division and their arrangement for diffuse action. "The extensiveness of the distribution of the fibers

of the sympathetic division is one of its most prominent characteristics" (**2**, p. 25). "Both from anatomical and physiological evidence the nervous relations in the sympathetic division of the autonomic system seem devised for wide-spread diffusion of nervous impulses" (**2**, p. 27). The mechanism is thus provided for affecting all of the viscera simultaneously in one way or another through stimulation of the sympathetic system.

The biological significance of this dual nerve supply to the important digestive organs has also been suggested by Cannon (**2**, p. 33). He indicates that efficient activity of the organism as a whole depends upon the reciprocal innervation of these antagonistic divisions of the autonomic system, just as orderly functioning of the musculature of the body depends upon reciprocal innervation of the antagonistic muscles. When the organism is confronted by an emergency such as might cause fear or rage, it has been demonstrated that the sympathetic system is temporarily dominant and that the impulse passing through these nerves causes inhibition of all digestive processes. During this process adrenin and extra sugar are poured into the blood stream as it is diverted to the brain and muscles. When, on the other hand, the organism faces no such emergency, the cranial division is dominant and the digestive processes are free to carry on their ordinary activity of building up reserves for growth and future emergency.

In our work with human secretions we have been able to corroborate this theory so far as the relationship of an emotional state to inhibition is concerned. During the observation of variation of parotid secretion in children, a shocking situation of some sort would be arranged without the subject's knowing of it. When a loud sound was made or the cry of 'fire' was heard the subject would show the usual evidences of anxiety or excitement and the secretions would cease to flow. More recent investigations suggest that even a mild affective disturbance such as the anticipation of distasteful food or of an electric shock is accompanied by partial or total inhibition (**7**). Even keen interest on the part of the subject

in mechanical puzzles has been noted to evoke temporary inhibition of the parotid glands. This dependence of secretion upon a favorable affective state finds ample support in the research of the Russian laboratories. Pavlov cites numerous cases of the inhibitory effect of investigatory behavior on the part of his dogs. The presence of a stranger in the room was frequently the cause of a failure to demonstrate conditioning. In his explanation of this behavior, however, Pavlov does not appear to recognize either the dual nerve supply to the glands or the functioning of a special system of nerves as being important in accounting for inhibition. In the last pages of his book he makes the following explanation of this process: "The results of these recent experiments incline us more and more to believe that the [2] *inhibitory process arises in the nerve cells themselves* and not in the connecting path between those cells excited by the conditioned stimulus and those excited by the special unconditioned stimulus employed" (4, p. 391). If this statement is not sufficient to indicate that Pavlov does not recognize the relationship of inhibition to sympathetic functioning suggested by Cannon, a further quotation from his final chapter makes clear the fact that he does not differentiate between muscular inhibition and secretory inhibition under an emotional situation, and apparently has not incorporated the antagonistic phenomenon of the sympathetic and parasympathetic system in his theories.

"Developing these conceptions further we are bound to regard the obsession of fear and different phobias as natural symptoms of inhibition in a pathological and weakened nervous system. There are, of course, certain forms of fear and cowardice, as for instance flight and panic, and certain postures of servility, which apparently do not conform with the idea of an underlying inhibitory process, having a much more active aspect. These types must, of course, be subjected to experimental analysis, but it is perhaps not impermissible to regard them as developing in co-operation with and as a result of inhibition of the cortex" (4, p. 410).

It is clear from this statement that Pavlov regards fear as

[2] Italics mine.

a [3] *symptom of inhibition* rather than as a cause; that he regards flight as not conforming with the idea of an underlying inhibitory process having a much more active aspect; and that he regards inhibition as cortical activity. There seems to be a marked contrast here with the theory of Cannon already stated. Pavlov thinks of inhibition as an underlying process which affects in much the same way the organism as a whole, so that muscular activity as well as glandular secretions are blocked. Cannon, on the other hand, considers inhibition as a function of sympathetic innervation, the immediate effect of which is temporary acceleration of the muscular and neural process and a blocking of the nutritive function only. Pavlov considers the cortex as the center of inhibition, while Cannon finds what appears to be convincing evidence that the thalamus is the center. The former attributes inhibition to cellular activity and the latter ascribes it to the functioning of a system of nerves.

These apparent differences in analysis of such a fundamental process by two such eminent physiologists deserves a more critical analysis than appears to have been accorded them. The common need for a better understanding of inhibition in psychology makes urgent some support for one or the other of these interpretations. At the present stage of our investigations the data seem to favor Cannon's theory. Not only is the generalized nature of the inhibitory response presented above in favor of his interpretation, but the nature of the reaction itself resembles known characteristics of sympathetic functioning. The most significant of these characteristics is the persistence of the effect of sympathetic discharge long after the original exciting condition has been removed. This characteristic is shown in the following account taken from Cannon. "Bickel and Sasaki started gastric secretion in the dog by sham feeding, and when the flow of gastric juice had reached a certain height, the dog was infuriated for five minutes by the presence of a cat. During the next *fifteen minutes* there appeared only a few drops of a very mucous secretion" (2, p. 10). In Pavlov's report of

[3] Italics mine.

studies on various phases of irradiation and concentration of inhibition in the analyzers, one finds ample evidence of this characteristic. "In the case of dog No. 1 the phase of irradiation of inhibition occupied about twenty seconds, while the concentration of inhibition occupied about seventy-five seconds. In dog. No. 2 the irradiation was complete after three minutes, while the whole inhibitory process in both its phases occupied fifteen minutes" (4, p. 161). The length of time taken for the recession of the inhibition would indicate a pronounced after-effect of the inhibitory activity. The fact that the evidence of this phase of inhibition was studied in the cortex is not conclusive evidence that it did not exist elsewhere in the organism. A subsequent quotation will show that there is a neural mechanism for irradiation from the thalmus to the cortex. Cannon (2, p. 49) and others maintain that adrenin secreted under sympathetic innervation becomes an agent itself of further inhibition of the digestive activities. Sympathetic functioning would thus automatically cause a prolonged after-effect.

The chief difficulty one encounters in an attempt to fit the data from the Russian laboratories to Cannon's theory of inhibition is that the latter states frequently that it is 'great emotional excitement' that interferes with the secretion, and there are examples of inhibition in the dogs when 'great emotional excitement' is not apparent. We have already shown, however, that in the case of human subjects mild affective states cause temporary inhibition, and Pavlov's dogs showed inhibition to such a slight disturbance as the sound of wind through a keyhole. So it seems unnecessary to require great emotional excitement as a prerequisite to sympathetic functioning. If a mild affective state can cause inhibition, then it seems to us that the cases of internal inhibition or experimental extinction which were used as a basis for most of Pavlov's studies could be accounted for. The technique employed in these cases, it will be recalled, is first to establish a conditioned response to a definite signal and then to repeat the signal at intervals without giving the dog his food. The question as to whether or not such a procedure would cause

an affective state cannot be answered with finality, but our observations of a dog in such an experiment were that he was upset. If one offers a dog a bone and then refuses to give it to him, does it not disturb his affective complacency? Certainly in the case of an infant, where evidences of affective disturbance are better understood, it would be generally agreed that failure to supply food after the signal of its approach had been given would cause at least a mild affective disturbance. Pavlov's dogs were always hungry and had all first been trained to expect food at a given signal before the study of inhibition was undertaken.

In this explanation of inhibition we merely challenge the differentiation Pavlov made in the beginning of his research between internal and external inhibition. If the dogs' behavior, which was ascribed to internal causes, can be shown to be due to indirect or external causes, then the basis for differentiation breaks down. Pavlov admits in his chapter on new interpretations that his former classification of inhibition might not have been well founded. "Hence a fundamental identity of all the three types of inhibition appeared very probable and special efforts were directed to the collection of further evidence on the point" (4, p. 387). It is clear from his discussion that he is finding it possible to explain his cases of so-called internal inhibition on the basis of external inhibition. His earlier description of external inhibition is contained in the following sentence: "In the latter cases inhibition of the activity of a given center is brought about as the result of an activity of some other center, which activity has arisen in its turn as a result of excitation from some afferent nerve or from some change in the composition of the blood" (4, p. 44). If this 'other center' were a subcortical one the two theories would seem to be very close together, but it is apparent from most of Pavlov's discussion that he is thinking of other cortical centers. It is to this point of the location of the center for inhibition that we now turn. Pavlov, as indicated, claims that it is a cortical phenomenon. Cannon's theory seems to be that it is primarily thalamic.

It is a common observation that the cortex exercises no

direct control over any of the functions of the viscera. The popular notion that one's mouth waters by merely thinking of food has never been demonstrated under controlled conditions. Neither can man inhibit these secretions by voluntary determination. One may put himself in a situation that will either excite or inhibit secretions but he is unable to do it directly. In the same way one cannot develop a state of anger by his own voluntary effort, but he might of course control situations which would cause him to become angry. Observation of decorticated dogs supplies conclusive evidence that the emotions are controlled in the subcortical areas. "Bechterev has reported that in an animal freshly deprived of its cerebral hemispheres, petting may call forth signs of pleasure; *e.g.*, purring in the cat and tail wagging in the dog" (**2**, p. 248). Many examples of subcortical control of rage or fear are cited by Cannon and it appears fairly evident from these data that both the bodily activity and the visceral changes characteristic of emotional activity are controlled in the ancient portion of the brain. The significance of these processes in the survival of the organism suggests the logic of their being a process of the paleëncephalen rather than the nueëncephalen. Undoubtedly Pavlov would agree that the emotions or affective states are subcortical activities, but so far as I have analyzed his reports he has nowhere connected them directly with inhibitory process except as cited above in abnormal cases. When he considered the possibility of inhibition being a subcortical phenomenon it was as the negative collateral aspect of a process of excitation without reference to affective processes.

In his discussion of negative induction, Pavlov did consider this possibility of inhibition being a subcortical process. "This investigation is, however, somewhat difficult, since it is necessary to determine whether the external inhibition is a cortical phenomenon, or whether it takes place entirely in the lower centers of the brain, seeing that in the case of external inhibition the centers of two different unconditioned reflexes are involved. From what is known with regard to the functions of the lower parts of the central nervous system,

it must undoubtedly be assumed that in the case of external inhibition of conditioned reflexes an interaction of an inhibitory character does occur between the centers of the two unconditioned reflexes" (4, p. 201).

In the light of the quotation above, showing that he admits the identity of the various types of inhibition under the head of external inhibition, Pavlov's admission here is extremely significant. From his final sentence we must conclude that he also thinks of inhibition as a subcortical phenomenon at least part of the time. It is difficult to harmonize many of the references in the book to cortical inhibition with this statement.

In conclusion it seems justifiable to suggest that Pavlov's inference that inhibition is primarily a cortical function due to cell changes and not due to the connecting pathways, appears to be questionable. The interpretation of Cannon that inhibition is a sub-cortical activity due to the functioning of the sympathetic system under thalamic control seems to be well founded by significant experiments and observations. It is supported by much of Pavlov's data as well as by his later interpretations. According to this theory the entire sympathetic system would be involved in secretory inhibition, and the data presented showing the widespread nature of the conditioned response as well as the characteristics of the reaction support such a claim. The conditioned salivary reaction, either positive or negative, is but a component of a larger response involving a division of the autonomic system. Of course, the cortex comes by the very nature of its function to play a dominant rôle as signalizing agent for the release of one or another of these divisions, but this acquired function represents a secondary rather than a primary process. Since unconditioned inhibitory activity is admittedly sub-cortical, conditioned inhibitory activity represents the association of some cortical center with this underlying mechanism. Pavlov has shown that almost any area of the cortex may be thus connected with either positive or negative processes. A certain noise in the underbrush, to which the child appeared indifferent at first, comes to be the signal for 'rattlesnake' and

now causes inhibition, not necessarily because of any difference in the cells of the cortex but because of its having been associated with a subcortical inhibitory process. The occurrence of the sound just prior to or concurrent with a negative reaction of greater potential caused the sound stimulus to be drained or inducted to the response of greater potential in the sub-cortical areas. Subsequent recurrence of this sound follows this same pathway for a short time at least.

With the adoption of such a theory many of the difficulties that have arisen in attempts to condition secretions in man and dogs may be explained. The almost universal failure of investigators to condition the parotid secretions of human subjects is perhaps due to their failure to consider the dependence of this process upon favorable autonomic conditions. Unfavorable affective states expressed in moods, attitudes, or emotional states could, and perhaps have, blocked the development of the response. In our own attempts at conditioning it was not until cognizance was taken of this factor that we were successful in developing positive conditioned salivary responses in human subjects.

Summary

Little consideration appears to have been given to the nature and complexity of the conditioned secretory response. An analysis of investigations and observations made with subjects where it was possible to observe quantitative changes in the secretion of two or more of the digestive glands at the same time, seems to warrant the assumption that the positive or negative conditioned salivary response represents but a component of an extensive and complex reaction involving all of the major nutritive glands. The nature of these reactions suggests a close relationship with the antagonistic divisions of the autonomic nervous system. The increase in saliva when food is seen may be the result of innervation of the cranial nerve supply, while inhibition of conditioned secretion may be due to sympathetic innervation.

In the light of these facts the theories of secretory inhibition advanced by Cannon and Pavlov are compared. Recent

investigations made in our laboratory, showing the inhibitory influence of mild affective states on parotid secretion, seem to support Cannon's theory that inhibition is of sympathetic origin and is controlled in the optic thalamus, as against Pavlov's theory that the inhibitory process is cortical in origin and is the result of cellular activity rather than the functioning of a system of nerves. Many of the difficulties encountered by investigators of conditioned salivary responses could be explained if this close relationship of mild affective states and inhibition were assumed.

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CONDITIONING AS A PRINCIPLE OF LEARNING

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A theory of learning, to be effective, must account for the facts which have been established. The significance of the facts themselves is, unfortunately, not at present clear. Most experimental investigations have necessarily measured learning in terms of certain end results, however these were accomplished, leaving undetermined whether the subject has learned to do one thing in one way or one thing in many different ways. And most experimental investigations have concerned such intricate special activities that their results cannot be generalized. Theories of learning can attempt to systematize only the facts of the more elementary forms of learning.

Most of the facts of learning are derived from common knowledge. The elementary facts of learning were fairly well understood by the associationists. The role of experiment in this field is as a rule illustrative. Pavlov's 'conditioned reflex' is accepted because it fits into a body of common knowledge about human and animal nature. That burned children dread fires is accepted before hearing of Pavlov. If Pavlov's results had contradicted this body of common knowledge they would have been met with sceptical analysis. Even though they seem to corroborate common knowledge they deserve thorough examination.

I

The following paragraphs will serve to call to mind the more important and established facts of learning. The summary is necessarily incomplete because it will be restricted to such facts as are recognized by common consent.

1. *Conditioning.*—Stimuli which accompany a response tend, on their recurrence, to evoke that response. Sometimes called 'association by contiguity in time' or 'redintegration'

or 'associative memory.' This generalization has long been recognized.

2. *Inhibitory Conditioning.*—Stimuli which tend to call out a response may lose that tendency, or, if they occur without the response, go further and acquire inhibiting effects. 'Negative adaptation' is the term proposed by Stevenson Smith and the writer a number of years ago for this characteristic of learning. The term 'conditioned inhibition' has also been used. 'Inhibitory conditioning' is perhaps better than either.

3. *Remote Conditioning.*—So little attention has been given to the precise time factors in conditioning that conditioning has generally been taken to include those cases in which the conditioning stimulus does not immediately precede its response, but may, an interval intervening, precede it or even follow it. According to Pavlov the interval in delayed and trace conditioning may be as great as thirty minutes. Pavlov would deny that the conditioning stimulus may follow its response, but studies in backward association would indicate that something resembling conditioning may occur in man when we have the time order, first response, then stimulus.

4. *Effects of Practice.*—Practice sometimes makes perfect. Repetition of a sequence of stimulus and response seems to establish the certainty of the sequence more firmly. 'Laws' of frequency or exercise are included in most theories of learning.

5. *Forgetting.*—The effects of learning seem to disappear at a somewhat predictable rate. The 'curve of forgetting' discovered by Ebbinghaus for nonsense material applies with variations to many forms of learning, characterized by rapid disappearance at the interval just following the last practice and a more gradual disappearance as time goes on. There are some odd exceptions to this general form of the curve, however. It is reported that the disappearance of learning is affected by intervening activity. After a period of sleep retention is better than after a period of waking. After a period occupied in a quite different activity, retention is better than after a period in which the situation is like (but not too like) the situation during learning.

6. *Temporary Extinction.*—In direct contradiction to the preceding generalization, Dunlap and Pavlov have pointed out cases in which the repetition of a habit sequence serves to disrupt the habit. Dunlap reports the successful use of practice to break up annoying habits. Pavlov reports (and his report is well verified by common human experience) that conditioning will disappear if a conditioning stimulus is repeatedly given at short intervals without the support of the unconditioned stimulus. After an interval the habit will be found somewhat restored.

7. *Emotional Reinforcement.*—The familiar fact that learning is facilitated by states of general excitement has been illustrated by recent experiment showing that nonsense material is better learned by subjects under slight muscular strain than by relaxed subjects. We may associate this characteristic of learning with what has been called dynamogenesis, the facilitation of learning by adventitious stimuli which might well be expected to diminish learning.

8. *Irradiation.*—Lacking a better term we may use the one offered by the translator of Pavlov for a phenomenon which he describes and which is easily demonstrated for human behavior as well as for animal behavior. After a stimulus has been established as a conditioner of a response it may be found that other stimuli to the same class of receptors or, in some cases, to different receptors, are able to elicit the response, though they lacked this connection before the experiment. With a touch on the flank used in connection with acid as a stimulus for salivary secretion, touches on other parts of the body prove less effective as their distance from the flank is increased. With practice of the original combination this conditioning power of neighboring stimuli tends to disappear.

9. *Patterns.*—Having learned to read, we find that reading appears to be more or less independent of the actual distance of the print from our eyes, and this means that similar patterns of stimuli are effective without reference to the particular receptor elements stimulated. The Gestalt psychologists assert that this response to patterns as such occurs

without learning. It is here included as a characteristic of learning because the writer believes that it is dependent on learning. Reasons for this belief will be offered later.

10. *Insight.*—As in the case of reaction to patterns as such the Gestalt psychologists have pointed out that the higher animals and man occasionally meet a new situation with an adequate and new response. The process of trial and error made so much of in recent behavioristic accounts of learning seems to be omitted in many acts, which are characterized by the Gestalt psychologists as cases of insight. This fact is here listed among the characteristics of learning because it is also the author's belief that insight is dependent on learning. Reasons for this belief will be cited later.

The experimental literature suggests many more characteristics of learning. Most of these, however, are either debatable or are complicated with factors other than learning which make their significance for a theory of learning slight. The experiments on distribution of practice are complicated by fatigue. The results in whole and part learning are ambiguous and depend on the nature of the material. So-called 'reminiscence,' in which children prove to be able to reproduce more material after an interval than at the end of practice, has not so far been rid of the suspicion of continued unrecorded practice. A theory of learning which undertakes to explain all the suggested facts is apt to be caught predicting results which do not occur.

Is there a single formula which can be made to include all or most of the established generalizations concerning the nature of learning? If there is such a formula it will in all probability be some form of the ancient principle of association by contiguity in time, which has been a part of all theories of memory and learning since before Aristotle, and has retained its essential character in spite of a variety of names, such as 'conditioning,' 'associative memory,' 'redintegration.' The remainder of this paper will consider the possibility that the facts of learning may possibly all be cited as instances of simple conditioning.

In order to examine its possibilities, the principle of

conditioning may be stated in a simple form: *Stimuli acting at a given instant tend to acquire some effectiveness toward the eliciting of concurrent responses, and this effectiveness tends to last indefinitely.*

The phrase 'tend to acquire' is used instead of 'acquire' because we have no assurance that this acquisition always occurs. It is the contention of this paper that this acquisition, when it does occur, is the fundamental mode of learning. The presumptive changes which make this re-routing of impulses lasting are the physiological basis of learning.

The principle is deliberately formulated to apply only to the momentary event. It is assumed that the phenomenon occurs during that small fraction of a second occupied by the conduction of an impulse through a center.

One more remark needs to be made concerning the language of the principle. The word 'stimuli' need not be taken in the sense of elementary stimuli to the individual receptor cells. It seems quite probable that patterns of such elementary stimuli may act as functional units and be subject to conditioning as units, that the elementary stimulus group *ABCD* may as a group excite a group of pathways *P*, while another stimulus group, *AEFG*, excites pathway *Q*. Conditioning redirection at remote association areas would affect these stimulus groups as functional units and not as elements. The stimulus *A* would be a conditioner of one response element through *P* and a conditioner of another, possibly an antagonistic element through the pathway *Q*.

The notion that such stimulus patterns might act as functional units *is not to be confused with the suggestion of the Gestalt psychologists that the patterns might act as functional units without reference to the receptors excited.* That is a very different matter.

We may now undertake an examination of the facts of learning in the light of this formulation of the ancient principle of association or conditioning. This amounts to an attempt to describe all of the forms of learning mentioned in the beginning as instances of the first, namely, simple, simultaneous conditioning. *This does not at all mean that learning*

is described in terms of conditioned reflexes. This phrase assumes a fixed unit of behavior which is organized in stereotyped form. It tends to obscure the fact that the behavior of an intelligent organism at any instant is a resultant of the total stimulus situation including internal stimuli. Reflexes and responses are never twice alike, because the total stimulus situation is never repeated. We may, at the outset, distinguish a theory of learning in terms of *conditioning* from a theory of learning in terms of *conditioned reflexes.* We have made no assumptions concerning the elementary acts which comprise behavior.

II

The characteristics of learning mentioned at the beginning of the paper were: 1. Conditioning; 2. Inhibitory conditioning; 3. Remote conditioning; 4. Improvement by practice; 5. Forgetting; 6. Temporary extinction; 7. Emotional reinforcement; 8. Irradiation; 9. Response to pattern as such; 10. Insight. We may attack these in order.

1. *Conditioning.*—This is, of course, the principle itself, and hence needs no reduction. Our inquiry concerns the possibility of reducing the other facts of learning to instances of this. We may then begin with the second.

2. *Inhibitory Conditioning.*—The circumstances under which a stimulus combination which has previously elicited a response will lose the power to evoke that response may be briefly stated. If the stimulus combination occurs, and the response is prevented by any means, the stimulus combination loses its power to elicit the response and, if the situation is repeated, will acquire a positive inhibiting effect on its former response. The response may have been prevented by inhibition from incompatible responses which prevail because the conditioning combination is weakened. Inhibitory conditioning is essentially the conditioning of inhibiting responses, and behaves like other conditioning in that it shows the effects of practice, is subject to forgetting, and so on.

3. *Remote Conditioning.*—In Pavlov's experiments a new stimulus is presented several seconds or minutes before an unconditioned stimulus, and then acquires the power to elicit

the response unsupported, with a latent period corresponding to the interval used in the experiment. Pavlov's explanation attributes this delay to mysterious latencies in the nervous system. He supposes the impulse to be somehow 'held up' in the cortex. This assumption is quite unnecessary. Like Bechterev, Pavlov tends to forget that his experimental animals have sense organs which are stimulated by their own movements. When the bell rings the dog responds by 'listening,' which is a series of movements, postural changes, turning of the head, pricking of the ears, and the like. When the salivary glands begin to secrete, the accompanying stimuli are not furnished by the bell but by these responses to the bell. The direct response to the bell is probably over in a small fraction of a second. After that the dog is responding to his first response to the bell. Just as when we answer the telephone we are, strictly speaking, answering the telephone only for an instant. After that we are answering ourselves, answering our start to answer the bell.

Such an explanation would account for a number of features of the 'delayed' and the 'trace' reflex. These are subject to inhibition or sudden release by new stimuli. What these new stimuli probably do is to alter the regular series of movements which comprise listening and the gradual recovery from listening. Delayed and trace reflexes are probably not direct conditioning at all. The true conditioning of saliva flow is on a stimulus pattern which follows the bell and is a consequence of the bell.

The apparent separation in time of a conditioning stimulus and its response is then quite possibly an illusion, and the assumption that responses to stimuli are either immediate or else do not occur at all is quite in accord with Pavlov's facts.

In delayed and trace reflexes the conditioning stimulus precedes the unconditioned. Pavlov states that no conditioning occurs if the stimulus to be made a conditioner follows the unconditioned stimulus. In a University of Washington experiment soon to be published something strongly resembling such 'backward' conditioning was found with human subjects. The results of experiments on backward association

suggest this also. This form of remote conditioning may also be in reality based upon simultaneous conditioning. No acts are instantaneous. Contracted muscles, through their own sense organs, tend to maintain their contraction. A new stimulus which follows the stimulus for a particular act may easily be simultaneous with the proprioceptive stimulation involved in the act itself, and hence become a conditioner of the act.

4. *The Effects of Practice.*—Improvement as the result of practice is a familiar fact. The increased certainty of successful performance which results on repeated practice has led numerous psychologists to the notion that the attachment of a conditioning stimulus to its response is somehow increased by repetition of the sequence, which is a very different matter. Improvement demands more *detachment* of stimuli *from* responses than *attachment* of stimuli *to* responses. In order to improve in a performance the awkward, embarrassing, misdirected movements must be eliminated and replaced by movements which lead to a successful outcome. At the end of training the individual must be doing something quite different from what he was doing at the beginning of training.

The assumption that a stimulus-response sequence is made more certain by repetition has been embodied in a number of 'laws of exercise' or 'laws of frequency.' It is quite possible, however, that the assumption is a mistaken one.

Pavlov's results in experiments in conditioning seem at first glance to indicate unambiguously that a conditioning stimulus is 'established' by its repetition with a stimulus combination which elicits salivary flow, and to indicate that the 'strength' or certainty and lasting quality of its establishment is a function of the number of occasions on which the two stimuli have been paired.

These experiments may be given a quite different interpretation. Conditioning, so far as elementary conditioning stimuli are concerned, may be an all-or-nothing affair, analogous to the setting of a switch, and not analogous to the wearing of a path, which has been a favorite simile. The increased certainty of response following on a given

stimulus situation may involve an increase in the number of conditioners, rather than an increased 'strength' of individual conditioners. In Pavlov's experiment, for instance, the bell signal results in extensive movements of orientation and postural adjustment, each movement causing appropriate stimulation to proprioceptors and exteroceptors. These movements are not identical each time the bell is struck because they depend in part on initial posture as well as on the bell. Repetition of the bell may enlist an increasing number of postural and other reflexes as conditioners of saliva flow, and hence gradually increase the certainty that salivary secretion will follow the bell.

It is entirely possible that if Pavlov could have controlled all stimuli instead of a very few, conditioning would be definitely established with one trial instead of fifty or more. The writer suggests that it is quite plausible that the more nearly such a complete control is established, the more nearly certain will be the result of the bell as a conditioner. Pavlov's whole method and experience suggest this.

The 'strengthening' of a stimulus-response connection with repetition may very possibly be the result of the enlistment of increasing numbers of stimuli as conditioners, and not the result of the 'strengthening' of individual connections.

5. *Forgetting.*—The conception of forgetting presented in the text-books has been that the effects of learning tend to be dissipated by some sort of physiological change at synapses which is a function of time. The form of the forgetting curve, though it differs for different sorts of material, indicates that forgetting is comparatively rapid when practice is discontinued and that the rate regularly diminishes.

There are some signs of a shift of opinion toward an explanation of forgetting in terms of conditioning. Hunter, in his article on learning in the recent volume of 'The fundamentals of experimental psychology' quotes with approval the statement of Jenkins and Dallenbach in an article on 'Obliviscence during sleep and waking' that "the results of our study as a whole indicate that forgetting is not so much a matter of decay of old impressions and associations as it is

a matter of interference, inhibition, or obliteration of the old by the new."

The evidence that forgetting is to be explained in terms of new conditioning is of several kinds. Forgetting is radically affected by intervening activities. If the intervening situations are materially different from the practice situations forgetting is less evident than when a certain amount of similarity holds of the situations. Probably the stimuli which are repeated while new responses prevail lose their conditioning effect on their previous responses and become conditioners of the new responses, and consequently inhibitors of the original ones. Furthermore, forgetting during a period of sleep is less than forgetting during a period of waking activity. This seems to be readily explained in the same terms. The stimuli which had become conditioners of certain responses are not repeated during sleep, and have no chance to be alienated from their attachment to these responses. During a period of waking, multitudes of stimuli from postural adjustments, movements, or from exteroceptive sources, which had been made conditioners of the activities in question are components of new situations and become conditioners of new responses. If we accept evidence from outside the laboratory we may quote those instances of vivid and detailed memories conserved for many years, which would, incidentally, have depended on one conditioning occasion rather than a practice series. The occasion for the restoration of such memories is probably an unusually complete restoration of situation, usually aided by such an absence of present distraction and inhibition as is found when we are on the border of sleep. Marcel Proust has described a common experience in which a memory evoked while lying in bed with closed eyes has persisted until a change in posture dissipated it completely. Association *may* occur after one connection, and *may* last indefinitely.

If forgetting is to be explained as new conditioning which replaces the old, how is the form of the curve of forgetting to be explained? Is it not entirely possible that the increased uncertainty of a conditioned response to a stimulus situation

is due to the progressive alienation of conditioners from their response, an alienation explained by their acquisition of new allegiances? The curves of forgetting may owe their shapes to the cumulative effect of this alienation. Since the bulk of the conditioners are probably proprioceptive, the result of the organism's own movements, the activities following on a given case of conditioning would alienate whole regiments of conditioners at the start and a decreasing number as time elapsed, because there would be a decreasing number to eliminate.

This statistical decrease in conditioners with time which is described by the forgetting curve would resemble, to use a frivolous illustration, the decreased expenditures of a certain artist whose method of protecting himself from starvation was to change the proceeds of his rare sales into dimes and broadcast these about his large and disordered studio. The following day dimes were retrieved easily in numbers. As time went on more and more search was required, though he seldom reached such a pass that an afternoon's search would not yield a dime.

These last faithful dimes resemble the last faithful conditioners which are indicated by the failure of forgetting curves to reach the zero point. The fact that some forgetting occurs during sleep may be due to the fact that some activity occurs during sleep, and hence some chance for the alienation of stimuli.

This conception of forgetting explains forgetting entirely in terms of new conditioning. It is, of course, not denied that there may be physiological changes like those in senility which do result in the deterioration of memory, but the normal occasion of forgetting is the alienation of cues following the occurrence of these cues at times when their conditioned responses are excluded by the general situation.

6. *Temporary Extinction.*—Pavlov's temporary extinction and Dunlap's Beta law are also conceivable as instances of the general principle of conditioning. They might be described as forced forgetting.

When an established conditioner is repeated without the

unconditioned stimulus, why should it quickly lose its conditioning power? Pavlov connects this loss with the brevity of the interval between applications. On his own showing this is not the determining factor. It is rather the *number of times the unsupported stimulus is repeated* that determines the extinction. Pavlov conceals this from himself by recording the results in terms of elapsed time from the start of the experiment. With short intervals, less elapsed time is required to extinguish, but in the case of both long and short intervals the number of applications is approximately the same.

If temporary extinction depends upon the number of times the conditioner is applied without support from the unconditioned stimulus, it is possible to explain temporary extinction in terms of the general principle of conditioning.

It should be noted, in the first place, that this temporary extinction or tendency to disappear with repetition is not an absolute generalization as it stands. It represents an exception to the rule of frequency. Sometimes one of these effects occurs, and sometimes the other. Obviously, in those cases in which temporary extinction prevails a special condition must have held. This special condition is probably what Pavlov asserts, the withdrawal of the unconditioned stimulus. It is the unconditioned stimulus that represents the most powerful determiner of the response. With the unconditioned stimulus withdrawn only occasional combinations of conditioners elicit the response, for it should be remembered that the animal is, in spite of sound-proof room and uniform lighting, in constant motion and subject to a continuously changing pattern of stimulation. At times there are more conditioners present, and at other times fewer. When the response fails, or is diminished because relatively few conditioners are present, these and other stimuli present become inhibitors, or, what is the same thing, conditioners of other responses.

There remains to be explained the reëstablishment of the conditioned response after a lapse of time. We have a hint toward this explanation in the fact that a sudden extraneous

and unusual stimulus may cause the conditioned response to recover its original strength and certainty. It is possible that the inhibiting stimuli in this case include the somewhat specific details of posture and environment which hold during the process of extinction. A sudden interruption disorganizes posture and orientation, removes many recently conditioned inhibitors, and allows the original posture and conditioners to prevail again. The reflex is restored much as a baulky horse is startled out of his baulking, or a man who has built up an obstinate attitude may be shaken out of it by a sudden change in situation.

7. *Emotional Reinforcement and Dynamogenesis.*—Explanation of the facilitating effect of exciting emotion on learning cannot be complete until a satisfactory physiology of the emotions has appeared. In the meantime attention may be called to the fact that exciting emotion involves general muscular tonus, and may possibly consist very largely in such increase in general tension. The physiologists have described many types of muscle-to-muscle reflexes which are excited by muscular contraction, the stretching of a muscle, or resistance to the contraction of a muscle. Intense stimulation of one receptor field resulting in the contraction of a limited number of muscles results in the contraction of other muscle groups through such muscle-to-muscle reflexes. States of general tension may be built up by the 'reverberation' of impulses in this fashion.

The origin of such states of general tension probably lies in intense stimulation of some receptor field, or in obstacles to free movement. In such states of general tension the acts which 'go through' are more energetic and complete. They involve the stimulation of many proprioceptor systems which would be undisturbed by action not so energetic.

The increased stimulation would give opportunity for increased conditioners, especially since the excitement itself is subject to conditioned revival.

In what has been called 'dynamogenesis' we have possibly two ways in which the irrelevant stimuli may facilitate learning. The new stimuli may serve to increase general

tonus through the 'reverberation' which has been described above and so serve to make action more vigorous and complete; and they may also, through the tendency to deflection which constitutes conditioning, serve to reinforce directly the prevailing responses.

8. *Irradiation.*—What Pavlov describes as irradiation, namely, the acquisition of conditioning effect by neighboring receptors which were not stimulated in training, may well be the result of simple conditioning, instead of the result of a direct spread of an entirely speculative condition with an inexplicable delay to neighboring portions of the projection areas of the cortex. It has already been suggested that the bell signal is not the direct conditioner in any of Pavlov's experiments, especially when it is learned that by 'simultaneous' presentation he usually means sounding the bell some two seconds before the food is presented. In two seconds many things may have happened. In direct response to the bell the dog 'listens.' The act of listening may be much the same whether the signal is a bell or a whistle of another pitch. Since the real conditioners of the salivary flow are the movements of listening, and not the bell, the whistle may result in salivary flow.

To a touch on the flank the dog responds by shifting his posture. This shift furnishes the conditioners of the glandular response. To a touch on a nearby point the response is a shift in posture involving much the same muscle groups as in the first case. To a touch on a more distant point the response will be different. The decreasing effect of stimulation of more remote areas may be the consequence of the decreasing likeness of the postural adjustment.

If we accept this much, Pavlov's experiments suggest the reason why 'irradiation' decreases with practice, since it is explained that with practice there is an increasing tendency for listening movements or defensive postural adjustments to disappear and give place to the eating movements and the eating posture.

One feature of these experiments as reported in 'Conditioned reflexes' remains unexplained. This is the statement

that corresponding points on the two sides of the dog have exactly equal effect. Being unable to explain this, the writer may be forgiven for expressing some scepticism concerning the facts, which seem to have no analogue in human behavior.

9. *Patterns.*—That we do respond to patterns as such is not open to question. And this would seem to involve the complete breakdown of any theory of conditioning such as is being presented, for at varying distances the actual receptors and afferent paths activated by a visual pattern must be quite distinct. The fact, indeed, cannot be questioned, though it should be noted that it is not a general or uniform occurrence. The child who has learned to read the raised letters on his blocks will not ordinarily recognize the letters when he sits on them. The effectiveness of patterns applies only within very limited fields.

Is it not entirely possible that the method by which we come to recognize a face at different distances as that of one and the same person is essentially the same method by which we come to recognize the rear aspect of this same person as his own back? In this case of recognition there is no question of similar patterns, for the back of his head resembles his face less than his face resembles the faces of others. If we maintain an attitude, or repeat a response to an object while that object is the occasion of shifting stimulation and of new stimulus patterns the maintained response may be conditioned on the new stimuli. Our response to a person at different distances is the same, with differences appropriate to the distance. Why may we not attribute this sameness and this difference to the samenesses and the differences originally present in the stimuli furnished by our original behavior in his presence?

If we accept conditioning as an explanation for responding appropriately to a person on hearing his footstep, which offers a stimulation pattern quite different from the visual pattern to which we previously responded, why should we consider it mysterious that the appropriate response could be called out by the stimulation of a quite different group of visual receptors? The fact that they have the same pattern is irrelevant.

The Gestalt psychologists assert not only that we respond in similar ways to similar patterns, which we undoubtedly do, but also that we do this *without any opportunity for conditioning*, which the writer does not at all believe. In the case of the hen which performed its trick using the eye which had been blindfolded during learning it is entirely possible that the cues for the proper movement were not primarily visual, but were furnished by movements connected with vision before the experiment was begun. Animals and man both have movements of skeletal muscles congenitally associated with vision. These movements may be in part identical for stimulation of either retina. If the act is conditioned on these movements, it might be elicited from either eye, without regard to which eye entered into practice.

10. *Insight.*—Concerning insight as described by the Gestalt psychologists the writer has much the same opinion as concerning response to patterns. The facts which are reported are not to be questioned and are typical of the behavior of the higher animals. An important part of the report has, however, been omitted. If the behavior described as insight is asserted to occur without previous learning, the essential part of the experiment would be the control of previous learning, and in the experiments the histories of insight are conspicuously lacking. No new category of facts concerning learning has been shown to be offered by the behavior described as insight.

In the writer's experience, insight in animals and in man is the result of accumulated habit. It was not a strange coincidence that the most ingenious person ever at work in the local laboratory has been a practical engineer for many years. When this member of the staff solved with little hesitation problems which had baffled the writer, there was a choice of explanations. It could be said that one man had insight and the other none, which seemed the poorer explanation; or it could be pointed out that one man had had previous training and the other none, an explanation much more charitable.

Summary and Comments

This paper has attempted to show that the main characteristics of learning, to wit, conditioning, inhibitory conditioning, remote conditioning, the effects of practice, forgetting, temporary extinction, emotional reinforcement, irradiation, response to patterns as such, and insight, may all be understood as instances of a very simple and very familiar principle, the ancient principle of association by contiguity in time. If the paper were not already too long many minor details which are readily described as instances of conditioning might be added. In their explanations of learning, Pavlov, the Gestalt psychologists, and many others have not examined sufficiently the possibilities of our proprioceptive sense organs in the role of determiners of behavior, and have tended to place the whole burden of explanation on highly speculative characteristics of the cerebral cortex.

This paper should be followed by some consideration of Lashley's work which appears to be at first sight a challenge to a general theory of conditioning. In the writer's opinion this work offers no objection whatever to the concept of conditioning. It does suggest that the simplest acts involve the stimulation of multitudes of receptors, the neglected proprioceptors among others, and the activation of multitudes of cortical pathways. As a single example it might be suggested that what appears to be a visual discrimination may actually be conditioned upon movements which are reflex responses to visual stimulation through subcortical paths. But a proper consideration of Lashley's work would require many pages.

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A PRELIMINARY ANALYSIS OF THE NATURE AND EFFECTS OF INCENTIVES[1]

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The problem of the nature and influence of incentives is a part of the more inclusive problem of motivation. The latter term is commonly used in a broad sense to include anything that stimulates the individual as a whole to activity. As compared with stimulation, which is used in physiology as well as in psychology, motivation is a distinctly psychological term. Only those stimulations which excite the individual as a whole to activity are, strictly speaking, forms of motivation.

The would-be experimenter in the field of motivation is confronted by vague and subjective concepts which render objective analysis and quantitative procedure difficult. In a review, in the PSYCHOLOGICAL BULLETIN for August 1929, of the work performed since 1927 on the influence of special motivation upon the acquisition of skill, McGeoch notes that "the accelerating influence exercised upon improvement by the introduction of special motivating conditions of diverse kinds seems established by these and earlier investigations. . . . Before, however, precision can be hoped for, the concept of motivation itself requires an immense clarification. One can scarcely quantify intelligently before the measured qualities have been isolated." The driving power formerly ascribed to the soul is now often credited to the individual's instincts, habits, desires, interests or purposes. Behavior is explained by the desires and purposes which are said to be back of it, or a person is said to have acted because of his instincts or habits. In reality the person acted because of the presence of a certain

[1] This article is drawn from a Ph.D. thesis written at Syracuse University, under the guidance of Dr. Floyd Allport, on 'Differences in the work output of children when performing simple tasks under different incentive conditions.'

pattern of stimuli, usually partly internal and partly external, the form of the response alone being determined by inherited (instinctive) or acquired (habitual) nervous organization.[2] His acting in that particular manner may have been determined by his instincts or habits. Desires and purposes are symbolic representations of previously experienced motivating conditions; the symbolic representations usually possess something of the stimulating power of the original experiences.

It is generally recognized that human beings are dynamic organisms, by no means exclusively dependent upon external stimuli for their activities. The internal rhythmical processes of bodily metabolism furnish intero- and proprioceptive stimulations which tend to discharge into the effectors. These muscular activities, commonly known as spontaneous, themselves produce proprioceptive stimulations, probably especially intense after periods of relaxation such as sleep, which tend to discharge into further activity. These internal forms of motivation, of a periodically recurrent sort and universal throughout the human species, are usually called the great drives of human nature; or, stressing their conscious aspects, they are called wants or needs.

Of the stimuli in the external environment those of a specific quality, such as certain tastes or odors, or those impinging upon a comparatively large sensory area, or any of a very intense or novel nature, tend inherently to be followed by vigorous sustained activity lasting until the stimuli are no longer present or the organism has become adapted to them. Other configurations of stimuli, such as words for instance, may, through the familiar process of conditioning, acquire the stimulating value of either these, or the organic, configurations.

Incentives belong among the external motivating situations. They excite the individual to great, or at least increased activity. In an era of high material prosperity and fairly well-organized family relationships, recurring organic urges, apart from proprioceptive stimulations and the cor-

[2] It is true that a habit once started does contain a motivating force within itself, for the proprioceptive stimulations from one movement tend to excite the next movement of the habit.

responding need for activity, play a subordinate rôle in the motivation of the individual. The significant position some of them undoubtedly hold in the motivation of individuals at a bare subsistence standard of living, or under certain special conditions, has been largely relinquished in modern life to external incentives. In this age of high machine productivity most men can satisfy their elementary needs by a relatively slight expenditure of time and effort, involving only a very partial use of their capacities.

It is true that hunger, thirst, temperature maintenance, waste elimination, and sex are of fundamental significance in determining the direction which human activity shall take; as, for instance, the raising of food products, the building of water reservoirs and the spinning of cloth; but they are of comparative insignificance, with the exception at certain times of sex, as motivating forces stimulating any one individual to intense and persistent effort. Contrary to the commonly held notion, they are of comparatively slight importance as motivating factors in the life of a person living in our advanced state of material civilization and relatively stable family relationships.

The periodic organic stimulations can be made effective as motivating factors only in proportion as their culmination in consummatory or end reactions is made difficult. In a desert, for instance, thirst might be a form of motivation stimulating exertions of a strenuous sort, that would persist until an oasis had been reached; among arctic explorers, subsisting on inadequate rations, hunger might motivate activity of a similarly strenuous and persistent nature until the party had made its way to a food cache; a tramp suffering from the cold, as the autumn changed to winter, might be motivated to seek a job and to work until he had enough saved to purchase heavy garments; people caught in a small closed space where oxygen was becoming scarce would be motivated to vigorous and persistent activity lasting until they were exhausted or some way of getting at fresh air had been discovered. On the other hand, whenever the periodically recurring organic needs are equally periodically satisfied in the routine of life, they

cease to be motivating factors of any importance. Though the sex drive may motivate a young man, for whom marriage is at the time impossible for financial reasons, to exert himself to find a better position or to excel in the tasks at hand, once he is satisfactorily married his sex needs will probably be satisfied in a fairly routine manner irrespective of the extent of his professional or business exertions.

The organic needs play even less of a rôle as motivating forces in the life of the child, and hence in the educational process, than in the life of the adult. Sex urges are not yet present and the other internal needs are satisfied regularly in the family routine quite irrespective of the exertions of the child. (It is true, however, that proprioceptive stimulations, at least, are a continuous daily source of innervation tending to discharge into the accustomed effector pathways or into the so-called random movements of the child.) In so far as food is a motivating circumstance it is not because of its effects upon the stomach and the satisfaction of hunger, but because of its effect upon nose and mouth. We do not strive to get nourishment; some food will be served us as a matter of course, even if we be adults; but we do strive to get fragrant odors and pleasant tastes. It is the palate and nose that we have to exert ourselves to satisfy and not the stomach. Similarly, we do not need to strive to keep the body at a constant temperature; sufficient heat and clothing to serve that purpose come almost as a matter of course, as a birthright in this civilization; but we do exert ourselves to secure garments, such as colored silks, which will be pleasant to feel and look at, and which will arouse words and expressions of attention and praise from our fellows.

In our civilization incentives and other external stimuli, either actually present to the senses or carried in symbolic terms, have largely replaced the organic needs (except sex during the period of sex maturity preceding marriage or during unsatisfactory marital adjustments, and except the recurring proprioceptive stimulations) as the primary motivating factors in the life of an individual, exciting him to vigorous and sustained use of all his powers. In the measure as

provision is made for regular and almost automatic satisfaction of our organic needs, other motivating factors become the essential condition for the full exercise of human capacities.

Positive incentives are external situations which an individual will exert himself to bring about or maintain; once achieved such situations usually stimulate the individual to further exertion. Thus praise is an incentive in that a person will exert himself to bring about a situation in which he is receiving praise, and in that the words and expressions of praise themselves stimulate him to further effort. Some incentives are of a negative nature; they are situations, such as words or expressions of disapproval or blame, which stimulate the individual to exertions usually persisting until situations of a similar sort have been definitely avoided. Incentives differ from other external motivating conditions chiefly in that they can be introduced as spurs to effort in connection with most of the activities of life. A little child in distress may promote vigorous and persistent activity of a helpful sort from most human beings, continuing until the distress has been relieved, but this is not a type of situation which can be used universally and frequently as a motivating factor in connection with other activities; hence, though it is certainly a form of motivation, it is not, strictly speaking, an incentive.

The following represent objective statements of the more common elementary incentives:[3]

1. A goal, or evidence of progress toward a goal.
2. A mark of distinction, such as a medal or a title.
3. A material reward, such as candy or some golf sticks.
4. Other persons working at the same task, whose performance can be compared with one's own (rivalry).
5. Words, expressions, or attitudes of praise, approval or encouragement.
6. Words, expressions, or attitudes of blame, disapproval or reproof.

[3] No claim is made that this is a list of mutually exclusive incentives; on the contrary, incentives are not distinct from, not independent of one another.

7. Words, expressions, or attitudes of attention and recognition.

8. Words, expressions, or attitudes of deference, obedience or submission (power over others).

9. Novel stimuli, such as those impinging upon the discoverer or upon anyone engaged in work of a creative nature.

10. Persons or places frequently associated with any of the above (some forms of altruism, and performance of a task for its own sake).

These motivating situations, which we have listed as incentives, can be brought into action in connection with many activities in progress as a spur to more intense and persistent effort. They facilitate any activity in progress.

Like the periodic organic needs these external incentive situations are effective as exciters of vigorous and persistent activity in proportion (within limits) as their attainment is made difficult. If praise, for instance, is given a worker indiscriminately and often, it ceases to have effect; it comes to be taken for granted and loses its power as an incentive; the worker finds he receives it, whether he exerts himself or not. Since it is granted him gratuitously, he does not exert himself for it. Likewise, if he finds that at his usual rate of work he outstrips others, rivalry is not an incentive, for he is first anyway. To measure the stimulating power of an incentive such as praise, it does not suffice to give the members of a group words and expressions of praise over a number of trials and to compare their output with that of a control group. Such an experiment furnishes no clue as to how much the subjects would exert themselves, *if necessary*, to secure the praise. Giving praise indiscriminately for a number of trials does not indicate the full stimulating power of the incentive. At most it indicates how much the subjects think they have to exert themselves to get praise from the experimenter in question. The full incentive value of praise has been measured only when one finds out how much subjects will exert themselves rather than do without it. In an experiment with 35 children using multiplication problems as the material, and chocolate bars as the incentive, we ran two series of

trials, one during which the requirements for the chocolate were gradually increased to a very high level, and one during which they were gradually decreased from that level. The average of the maximum number of problems done by a child in any one trial of each of the two series was the measure of the stimulating value of a chocolate bar for that child; this average indicated the number of problems he would do rather than go without the chocolate.

Experimenters have frequently claimed to measure the stimulating power of isolated incentives, such as evidence of progress toward a goal, rivalry, or approval. In reality incentives are interrelated and it is well nigh impossible for an experimenter to introduce only one into his experimental setting. When several subjects are performing the same task and performances can be compared, the incentive of rivalry is present even though the experimenter has formally introduced only praise as an incentive. Simply to ignore certain incentives is not a safe method for insuring their nonexistence in the experimental setting. Incentives are interrelated; if one receives praise, one has probably also improved one's own record, improved one's standing with regard to others, and obtained recognition. There is no way of telling what has been the comparative rôle of these various incentives in the total stimulation of the individual. At most the experimenter has stressed a certain incentive by drawing attention to it; he has not isolated it; he does not even know to what extent he has indirectly drawn attention to other related incentives. He is not justified in ascribing his results to a particular incentive.

Incentives are not all interrelated in the same manner or to the same extent. A material reward may also function as a goal and a mark of distinction, but the last two cannot function as a material reward. Hence, to measure the incentive value of such a reward, for the performance of any type of work, it would be necessary to compare the output of two groups differing only in that one group was offered a goal and a mark of distinction whereas the other was offered, in addition, a material reward. Only under such conditions

could one obtain a measure of the stimulating power of the material reward, as such, apart from its incentive power as a goal and a mark of distinction.

Even were the experimenter to overcome the difficulties arising from the interdependence of incentives and were he to succeed in securing the full effect of the chosen incentive, unless his experiment were a very long one, he would still have thrown no light upon the long run use of his incentive. Frequent applications of an incentive might not all yield the same results.

Many incentives are complex, consisting of several of the more elementary incentives previously listed. Money, for instance, may at times be an incentive incorporating directly or indirectly almost all the elementary incentives: it may serve as a goal toward which one's efforts are directed, or as a mark of distinction; it may be the means of securing some material goods such as cigars or an automobile; it may serve as the evidence that one has outstripped one's competitors; it may be the means of securing words, expressions, or attitudes of praise, approval, attention, recognition, deference and submission from others; and finally, it may enable one to afford novel stimuli, as in travel. The power of money as an incentive resides in the fact that it may represent to an individual the sum of most of the elementary incentives.

We have hitherto considered incentives as objective situations external to the individual. We may now inquire, what specific attitudes and behavior patterns do these external situations stimulate? In the experiment with children using multiplication problems as the task, we found that during the trials in which no incentives were used the children tended to slump in their chairs, to look out the window and about the classroom, to whisper to one another, to make faces, to drop pencils, and to sit completely motionless (relaxed), more frequently than during the incentive periods. During the latter, the subjects sat either more upright or more bent over their papers, their hand movements were faster, there were fewer vocal sounds and fewer head or eye movements toward other objects than the multiplication sheets; bodily

movements, apart from writing, were chiefly stretchings or small mechanical movements, such as raising and lowering the tip of the foot. There was approximately three times as much borrowing of erasers, pencils, and handkerchiefs, dropping of pencils and sheets and writing of notes during the no-incentive trials as during the incentive ones. The introduction of incentive situations into a setting in which a comparatively routine task, such as multiplication, was being performed, seemed to be followed by an increase in the muscular tension of most of the subjects,[4] by an absence of most movements not relevant to the task being performed, and by an increase in the speed of the movements used in that task. We will call this the incentive behavior pattern, or more briefly, the incentive attitude. This attitude may be present to a varying degree; but objectively there appears to be only one type of incentive attitude. We think of a variety of incentives, rivalry, praise, etc., because a number of situations will stimulate this incentive attitude; it is from those situations that incentives are named. It is correct, therefore, to speak of rivalry situations, of praise situations, etc., and of *the* incentive attitude stimulated by them. From the objective point of view an incentive is a situation which stimulates and maintains over a period of time the behavior pattern or attitude just described.

That a state of muscular tension may be more favorable for mental work than the 'normal' state, has been demonstrated by Mr. Bills.[5] When squeezing a dynamometer with a constant pressure his subjects learnt nonsense syllables, and added, more quickly than when performing these operations under 'normal' conditions.

Through the familiar process of conditioning, the number of elementary situations which can arouse the incentive attitude increases. At first the child may exert himself only for praise, toys and delicacies, but if he gets these when he outdoes others in competitive performances, the presence of

[4] This tentative statement regarding muscular tensions needs to be checked by the use of instruments measuring whatever tensions may exist during the use of incentives.

[5] A. G. Bills, The influence of muscular tension on the efficiency of mental work, *Amer. J. Psychol.*, 1927, 38, 227.

others performing the same task, or the sight of their scores, may become eventually effective stimuli for the incentive attitude; or the mere sight of his own scores, irrespective of those of others, may come to stimulate that attitude and finally perhaps the work alone will suffice to do so. Incentives are necessarily interrelated because they have been together in the past as elements in work and play settings. We found that the children in the multiplication experiment previously mentioned varied in the extent to which this conditioning process had gone, and therefore in the number of situations which would arouse the incentive attitude, and in the extent to which that attitude was stimulated.

Since it seems to involve muscular tensions, the incentive attitude should be fatiguing. This statement might be tested by comparing the extent to which fatigue products are present in the body when such work as mental arithmetic is performed with and without incentives. It seems probable that a high speed of performance obtained by the use of incentives might, as it became habitual, necessitate less and less muscular tension and be correspondingly less fatiguing. The rate at which a series of movements is performed has to be learnt, as well as the absolute sequence of movements, and each may become habitual.

When acquiring any new skill, such as that involved in sorting cards, a child has to learn both the sequence of movements involved and the rate at which they should follow one another. If he attempts to perform his task at a faster speed, in order to get a chocolate bar for instance (or, for that matter, at a slower speed), he has to learn the new timing. Professor Griffith, in a paper on 'Timing as a phase of skill' read before the American Psychological Association in 1928, emphasizes the fact that the timing of movements, or as he calls it 'the rate at which,' must be learnt as well as the pattern of movements. From experiments, in which subjects learnt to go through stylus mazes at various speeds, he thinks it probable that differences in the timing of movements may be more difficult to learn than their sequence. There is a neuromuscular pattern for each rate of execution of a move-

ment differing from the pattern for other rates of movement. Acquiring a skill at any particular rate consists really in acquiring a new bodily pattern. In a card-sorting experiment, for instance, the subjects may have learnt to manippulate the cards when holding their arms in a certain position and with a certain degree of muscular contraction in the arms and remainder of the body. If, when incentives are introduced, the muscles throughout the body become more tense, the movements involved in manipulating the cards have to be fitted into this new neuromuscular pattern. If the change is too sudden and radical the manipulative activities are disarranged and the subject has to start learning again. The skill has been superimposed on a new bodily set with which it is not yet integrated.

With our limited knowledge of nerve and muscular physiology it seems unwarranted to attempt anything but a very brief and tentative physiological explanation of the increase in speed of movement following increased muscular tensions. It may be that the augmented flow of nerve energy from kinæsthetic end organs results in quicker movements because of the quicker overcoming of synaptic resistances, resistances which may require a summation of individual impulses to be overcome. Whenever several sensory neurones lead to a common synapse, that synaptic resistance will be more quickly overcome when impulses are coming from many sensory neurones, than when they are coming from a few neurones, and there must be a summation of the local electrical or chemical effects before the synaptic resistance is overcome.

Summary

1. Incentives are external situations which individuals will exert themselves to bring about or maintain.

2. They differ from other external forms of motivation in that they can be artificially introduced into the settings accompanying most of the activities of life.

3. In an advanced material civilization with fairly well established family relationships, incentives have largely replaced the great organic drives as the most universal and powerful motivating factors in the life of the individual.

4. The full stimulating power of an incentive can be measured only by determining how much subjects will exert themselves rather than do without the incentive. We suggested a method similar to one used in determining sensory thresholds in psychophysics.

5. Since most incentives are interdependent, a special technique is required to measure the stimulating power which can properly be credited to one particular incentive.

6. It is probable that the effects of any incentive will vary, depending upon how long and how often it is used.

7. There are two aspects to an incentive: the incentive situation and the incentive attitude stimulated by that situation. The various incentives are names for the various situations which will arouse the incentive attitude.

8. We tentatively described the incentive attitude as consisting mainly of muscular tensions, which facilitate any activity in progress.

9. When incentives are introduced and a skill is performed at a greater speed, the new timing has to be learnt, just as originally the sequence of movements had to be learnt. The new timing means that the special movements involved in the performance of the skill have to be integrated with the new bodily pattern which we have called the incentive attitude. This attitude may, of course, vary in the intensity of the muscular tensions involved. The incentive attitude is a source of energy which can probably find outlet as well in more vigorous or persistent movements as in faster movements.

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FRAGMENTARY EIDETIC IMAGERY[1]

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In view of recent results it seems highly questionable whether the difference between fluctuating and stable eidetic imagery has the clinical importance which E. R. and W. Jaensch ascribe to it (**6**). Eidetic images viewed as 'optical stigmata' are supposed to be diagnostic tools. Hypothetically, fluctuating eidetic imagery is linked up with a 'cortiform (neëncephalitic) reaction type' while stable eidetic imagery is related to a 'subcortiform (paleëncephalitic)' reaction system (*cf.* **9**, pp. 84–90). There is no doubt that a large amount of experimentation will have to precede the general acceptance of these two biotypes. At any rate, we found that an examination of eidetic imagery in a large group of subjects yields characteristics which permit not only of a classification into fluctuating and non-fluctuating eidetic images, but of many other classifications. As regards the phenomenology of eidetic images we are unable to see why the classification proposed by Jaensch should be more important than any other based on differences in the behavior of eidetic images under experimental conditions. Etiologically, however, it is *possible* that one of the distinctions drawn on the phenomenological level is more important than others of the same nature. This exactly is the contention of E. R. and W. Jaensch. However, as long as the relation of eidetic imagery to general 'psychophysical reaction systems' is still in the experimental stage, we do well to give heed to any characteristic apt to throw light on the processes involved in eidetic vision. In the following (*cf.* **11**) we call attention to

[1] Some of the data utilized in this study were collected during the writer's tenure of a Social Science Research Council Fellowship 1926–28. The writer wishes to express his indebtedness to the Social Science Research Council.

[2] Studies from the Behavior Research Fund, Chicago; Ernest W. Burgess, Ph.D., Director. Series B, No. 165.

the mechanism of fragmentation in eidetic imagery, a mechanism observed while studying eidetic individuals of different races in different districts of America (California, New York, West Virginia, Ohio, Louisiana). At the very beginning it must be said that the fragmentary eidetic images we are considering here are neither identical with the fluctuating images of the B-type which often represent nothing but memory-images 'seen' in perceived space, nor with non-fluctuating eidetic images in the sense of Jaensch. We do not propose a classification into fragmentary and non-fragmentary eidetic images; we believe, however, that the mechanism of fragmentation is of sufficient importance to justify its description. The term 'fragmentary' is used here in view of the fact that after the removal of the stimulus object, the object is eidetically reproduced in certain characteristic fragments. To illustrate the behavior of such fragmentary eidetic images, some excerpts from our protocols will be quoted. Since it is the mechanism of fragmentation rather than the eidetic character of the images we are interested in at present, we do not give a detailed description of the tests used for determining eidetic imagery. These tests were similar to those described in our monograph (7) and in the Marburg publications (*cf.* especially 41–47 and 60–69 of the bibliography in 9). In the following, various kinds of fragmentation as found in eidetic imagery are illustrated.

One of the stimulus objects used in our experiments was a modified form of the silhouette published in our monograph (7, p. 139). It pictures a sudden commotion in the street caused by a boy running his cart, loaded with a large number of packages, against the curb. A large number of details are represented in this picture, *e.g.*, nine persons, three dogs, houses, trees, lamp posts, a church, and objects falling from the cart. In the experiments referred to, the subject is merely asked to look attentively at the picture (time of exposure: 60 sec.; distance of the stimulus object from the eyes: 50 cm.; no fixation required; eidetic image observed on a gray screen of Hering's paper; *cf.* 7).

Upon removal of the stimulus object from sight, we find

that the eidetic image of the picture seen by A. S., a Jewish boy of 11 years, consists of a 'green flag,' 'the red sign,' 'a yellow parcel,' 'blue ink' (correct 'elements' in the picture shown). Nothing else appears. Upon increasing the distance of the gray screen from 50 cm. to 100 cm., the following additional details are recorded: 'flagpole to the left,' 'stick where the red sign is,' 'ribbon around the parcel,' 'bottle where the ink is' (correct). After closing his eyes, a very complete eidetic image of the stimulus object is seen by the subject, but the description of this image consuming about ten minutes is of no interest in this connection.

The picture used here as a stimulus object shows two labels on one of the packages falling from the cart. On one label the letters L. O. B appear in black capitals. To the right below this label, a second label is visible. Being rather indistinct, we can only assume that it reads D. S. Both groups of letters are surrounded by black lines in the form of rectangles.

A Negro boy, H. P., 11 years of age, after having described his eidetic image of the picture with closed eyes, remarks that he can see 'some writing' on one of the packages but that he cannot read it. Suddenly a C appears. "Now it looks like a B." This B appears in a brownish color. Subject: "There are some more letters. There are some dashes underneath." At this point the experimenter illuminates the closed eyes of the subject by means of a flashlight: the B turns purple. The subject thinks that "there are three others" (meaning letters). Experimenter: "What does it look like before the B?" Subject: "It's round." Exp.: "And before it?" Subj.: "It looks as if it has a corner down at the bottom." After the experimenter's pressing slightly on the closed eyes with his fingers, the subject remarks: "The second letter looks like an O." Exp.: "And the first one?" Subj.: "It's just the same."

Thus, the final eidetic reproduction of L. O. B is (1) something with 'a corner down at the bottom'; (2) an O-like shape; (3) a B. The subject does not remember the correct lettering on the label.

The same picture used in this experiment shows to the right a house with two posters on the wall. The lower right corner of the larger poster is torn off. In the center of this poster RICHMOND appears in black capitals. Below this word some visual configurations arranged in rows are visible, probably indicating some kind of announcement although letters are not discernible. Above RICHMOND is another row of letters: C. U. R. F. C. All letters on the poster give the appearance of being printed by hand.

F. B., a Negro of 18 years, describes, first with open, then with closed eyes, the various details of the picture as they appear in his eidetic image. Thereupon he is asked by the experimenter: "Can you see the sign on the wall?" Subj.: "I see an O, but cannot make out the others." After a while the subject suddenly remarks: "I see the letters real good." Since he pauses the experimenter inquires as to what he sees. "I see R I" (the subject hesitates and, then, after a pause during which he has covered his closed eyes with one of his hands, continues) "and then something I cannot make out, and then an O. . . ." After rhythmical illumination of the closed eyes, the subject announces: "Now I see the whole picture again. And after the I there is a curve like this" (indicates a C-shaped curve with the index finger). After a rhythmical illumination of 10 sec. there follows the exclamation: "After that is an H." Upon continuing the rhythmical illumination on the eyes: "It's getting darker now. I don't see the H any more." Exp.: "Is there another letter to the right of O?" The subject informs us that he sees from the left to the right RI, then "a curve like a C, then an H," and then something he cannot make out, "and finally an O." "O is the plainest thing I can see. There are more letters above and below like dots." In this case the stages of eidetic reproduction may be summarized as follows:

O
RI O
RIC O
RICH O
RIC O
RICH O

The subject does not only fail to reproduce the whole word eidetically, but is also unable to recall it. Naming stimulus words likely to lead to the response 'Richmond' does not change this result. It is of interest that the letter M as it appears in the word RICHMOND on the real picture looks more like a rectangle with one side lacking ⊓ than a conventional M.

The house with the posters just referred to has an entrance above which the letters ENTR appear in black capitals the E being larger than the other three letters.

The subject above referred to remarks after having described with closed eyes his eidetic image for about 7 minutes: "There is a T above the door." Then, after having mentioned other details in his eidetic image: "I see ETR above the door." Exp.: "Which is larger, the T or the R?" Subj.: "No, the E is larger." Then a report on other details seen in the image follows. Suddenly the experimenter asks: "Do you still see ERT?" Subj.: "No, I see ETR." ENTR, therefore, is eidetically reproduced in the following manner:

T
ETR

That means that the position of the N is not indicated by 'empty space.'

The same letters, ENTR, appear in the eidetic image of G, a Jewish boy of 10 years, in the following way:

ENT▷
▷
ENT
ENT▷

In the first stage, as becomes evident from the drawings and description of the subject, there is 'a triangle' behind the three letters. Then this triangle 'wanders' to a place above the N and then returns to its place behind the T. At the same time, changes in size are reported. First, the letters are 'all small,' then 'all big,' then 'two small, the last big,' then 'the first big, the last two small.'

In some of our experiments cuts from *Münchener Bilder-*

bogen were employed. One of the pictures used represents a scene before a stand designated on the cut as *Trinkhalle*. The picture, in this case, was exposed for 15 sec. As before, no fixation was required. One of the subjects, a Jewish boy, St. S., 13 years of age, after having described his eidetic image, is asked by the experimenter: "Any writing?" "Oh, yes, I see a T." Upon 'concentrating' a while on the place in his image where the T appears, he remarks: "There is nothing. It's all white there." But finally other letters appear. The eidetic reproduction of the word *Trinkhalle* in the case of this subject may be represented thus:

T
Tri
Trin
Triu
Triun
Triunp
Triun
Triunp

At the end the subject sees Triunp, but remarks that the p is not clear. Illumination on the closed eyes as well as leading questions on part of the experimenter do not change this result: "I still see Triunp."

In general it may be said that in experiments in which pictures are employed as stimulus objects, fragments of the following kind may appear in the eidetic image: 'one eye—don't know whose eye it is'; 'shovel'; 'a stick with cross pieces'; 'a nose upside down'; 'quarter circles' (meaning curves in one row); 'blades of grass'; 'row of buttons'; 'a jacket with black dots'; etc. Very often these eidetic fragments represent 'elements' of the real picture but incorrectly *named*. Although the fragments may represent certain parts of the stimulus object they are often not recognized or remembered as such by the subject: "I don't think of them, they just come." Sometimes the fragments display various degrees of similarity to elements in the real picture or do not appear at all in the picture.

These excerpts from our records seem to be sufficient for illustrating the mechanism of fragmentation which we have in mind. The fragmentary eidetic reproduction recorded here impresses one at first sight as totally different from all forms of reproduction ever recorded. It becomes imperative, therefore, to relate our observations to, if possible, similar observations in the literature. With the exception of Schilder (**20**) it is, to our knowledge, only Urbantschitsch (**21**, **22**) who has reported the above splitting-up into fragments.

As far as Professor Schilder is concerned, it is necessary to recall his statement that he considers himself a very strong eidetic of Jaensch's B-type. He reports on an experiment in which he used as a stimulus object a picture representing a group of boys at play: to the left, ahead of the others, the leader of the group; he is followed by four boys, the first wearing a bluish-green apron, the second one a green, pointed cap, the third carries a drum, and the fourth a wooden sabre.

In his eidetic image, Professor Schilder first sees a red cap and a drum, the drumstick moving rapidly at considerable distance from the drum; four indistinct faces and something green. Concentrating to get an eidetic image with the characteristics of the real picture, the four figures become distinct and a green apron becomes clearly visible below the row of the boys. The observer feels that there must be something to the left: a wooden sabre appears in the field to the left. There is also a boy marching above the drum. His legs move frantically, and the trumpet he blows is moving back and forth.

We shall now shortly consider some of Urbantschitsch's findings. One of Urbantschitsch's subjects is shown, for instance, a picture representing among other objects a boy leading a dog with a rope. The boy holds one end of the rope in his hand. In the eidetic image, only the boy appears. Upon questioning, it is found that the subject remembers the boy only. Asked to look at the hand of the boy in the image, a line appears moving to the left and downward to an indistinct spot. After 'concentration' on this spot, it develops into the shape of a dog.

In another case, the subject is surprised to see in his eidetic image a black spot protruding from the wall of the house. It is found that the picture used as a stimulus object shows at the place of the black spot a crow commencing to fly.

The experiments in which Urbantschitsch used Miss Windhager as a subject (**22**) seem to have a more definite bearing on the problem of fragmentation. The conditions under which these experiments were performed are not quite clear from the author's report. A stimulus object unknown to the subject was moved quickly across her eyes at a distance of 10–12 cm. Immediately thereafter the subject had to close her eyes and to give a report about what she had seen, and then to describe her eidetic image.

In the 'fourth experiment' of Urbantschitsch, the words *Lilie* and *Glück* were written one below the other on a piece of paper. The subject upon closing her eyes remembered nothing but lines upon the paper. The eidetic image appeared as follows:

.
..
..e
.ie
i ie
Li ie
Lilie

Below the L of the word *Lilie* a C appeared, below the e an r. It was found that the upper portion of the G as written on the paper in *Glück* was similar to C and the lower part of k similar to r.

In the 'fifth experiment' Urbantschitsch himself had not seen the stimulus object. He moved it quickly across the eyes of the subject. Again, the subject remembered nothing but lines. The eidetic image as seen with closed eyes went through the following phases:

o
o o
o-9
6-9

679
°679
1°679
18679

The number actually exposed was 61879. The 6 and 9 were first eidetically represented by ciphers. Then, one cipher was supplemented by a curved line downward, the other cipher by a curved line upward.

In another case, when the subject again had seen nothing but lines while the experimenter had not seen the stimulus object before, the following stages appeared in the eidetic image:

6
° 6
o° 6
o°′)6
9°′)6
98′)6

′) was described as a curved line which the subject believed to be a part of a 3 or a 5. The number actually exposed was 9876.

So far as *facts* are concerned, the data on fragmentary eidetic imagery collected by us while studying eidetic imagery in general may be summarized as follows:

(1) There is a splitting-up of the stimulus into, very often relatively meaningless, details.

(2) Temporally, the fragments, after removal of the stimulus object, may appear after relatively long periods (several seconds, minutes or hours); *e.g.*, while observing a negative after-image, the head of a doll, the doll having been shown to the subject 20 minutes ago, may suddenly appear; the fragments of the image may disappear and then reappear.

(3) The fragments, while not intentionally influenced by the subject, may undergo changes of various kinds: rotational displacements; translocation of shapes, colors, directions, and movements; changes in size and color; changes from 'dots' to meaningful objects and vice versa.

(4) Upon activation of (so-called) attention, (*a*) missing elements may gradually or suddenly appear, or only parts of the desired element may appear; (*b*) elements may appear having characteristics not found in such a way in the stimulus object; (*c*) missing elements may appear, while others present in the eidetic image may disappear; (*d*) instead of the appearance of missing elements, only rotations or spatial displacements in the fragments present are observed.

(5) 'Disturbing' stimuli (noises, tuning-fork sounds, thermal stimuli, illumination or pressure on the bulbs, etc.), lead often to unexpected changes in the same sense as attention does.

(6) Elements as found in the stimulus may be present in the eidetic image but are not recognized by the subject, or only recognized after a long time as parts of the stimulus.

(7) An element of the stimulus object may be absent in the eidetic image, still its correct position is indicated by an 'empty' space. The 'nothing' somehow exists psychologically, it has a 'place-coefficient.'

So far as the *interpretation* of these facts is concerned it is not likely that this fragmentation can be entirely explained by reference to the properties of the stimulus objects used, such as size, color, etc., or by subjective factors, such as *Einstellung*, attention, or interest, etc. The factors which in our monograph (**7**, p. 197) we considered most important in determining whether or not a certain 'element,' *e.g.*, of a picture, is likely to be eidetically reproduced, are undoubtedly sufficient for explaining the behavior of Jaensch's fluctuating eidetic images in this respect. But we believe they are not sufficient for explaining the kind of fragmentation above described.

A further point is to be considered. In our experiments the details of the stimulus object were known to the experimenter. Still there is no necessity for introducing telepathy at this point. Aside from the considerations set forth by Urbantschitsch and the precautions taken by him (**22**), it must be said that introspectively we do not feel guilty of ever having consciously reproduced words, etc., in the above

fragmentary manner during the experiments. It would be necessary to assume not only the existence of a subconscious mind and telepathy, but also a fragmentation of the above kind in subconscious reproduction. We do not feel inclined to make any such assumptions.

It seems possible, however, to throw light on our problem by considering the mechanism of fragmentation as it is operative in phenomena which are of a non-eidetic character. In general, we may assume that many of the biologically significant mechanisms underlying our responses to visually appearing stimuli are independent of the distinction between visual imagery, eidetic imagery, hallucinations, and perception. Some time ago, in our book on mescal (**10**), we called attention, *e.g.*, to the fact that mescal produces diplopia and polyopia not only of hallucinated and real objects, but probably also of imaginal objects. It would be, indeed, of considerable importance if fragmentation in eidetic imagery could be related to a similar mechanism, *e.g.*, in perception.

Such similar splitting-up into spatio-temporal fragments is found in the perception of real objects in pathological cases. The study of aphasic, paraphasic, and agnostic cases has brought to light facts decidedly similar to those recorded by us. In certain pathological cases, as well as in the eidetic cases above referred to, the following facts stand out: (1) Fragmentation in spatio-temporal respect; (2) the influence of attentional processes on the fragments; (3) the disappearance and re-appearance of fragments, *i.e.*, rivalry phenomena; (4) rotations and displacements; (5) the appearance of film colors; (6) the existence of excessive simultaneous contrast; (7) scintillation. These facts merely enumerated here are cited in support of the view that the study of pathologically disturbed vision and of eidetic vision leads to a number of characteristics independent of the difference between eidetic imagery and perception. As regards the facts in the field of pathology, it is not possible to present even a condensed summary of the pathological literature in question. The reader is to be referred to the studies by Pötzl (**1**, **15–17**, **19**), von Stauffenberg (**23**), Head (**5**), Poppelreuter (**14**), Pick (**13**),

Hartmann (4), Gelb (2), Gelb and Goldstein (3), and to the widely scattered war literature on visual disturbances after cerebral lesions (8). To illustrate: Pötzl's case, Obszut, for instance, a case with a relative paracentral scotoma, is shown a yellow cross on a white ground. He sees nothing but the white ground. After the white ground and the cross have been removed and nothing but a black ground is visible, he remarks: "A cross—yellow" and indicates by a movement the form of the cross. Very shortly it may be stated that in pathological cases 'discontinuous' or 'atomistic' perception as well as the seeing of 'unimportant' details are reported very frequently. In fact, very often a fragmentation results which is decidedly similar to that occurring in eidetic reproduction.

In trying to explain fragmentary perception, different investigators have thought of 'attention' as the causal factor. They think fragmentation is due to a hyper- or hypofunction of attention, or to disturbances of 'passive' or 'active' attention, etc. This view is of more than passing interest since it cannot be doubted that an analysis of sensory defects brings to light the importance of non-sensory factors. Again and again, in the study of visual disturbances, difficulties have arisen in separating for instance 'functional' and 'organic' symptoms. Whether a 'tubular' field of vision is to be accounted for as an expression of hysteria or on an organic basis is often difficult to decide. Frequently it has been necessary to consider the interdependence of 'hysteriform' and 'organic' mechanisms in cerebral cases (18). Poppelreuter, for instance, has pointed out that both a hypofunction of perception and an 'insufficient attention' may lead to charting the same defect perimetrically. Again, 'insufficient attention' may mean hysterical 'weakness of attention,' or a disturbance of 'optical attention.' Poppelreuter goes even so far as to suggest that hemianopsia resulting from injury of the occiput may not be due to the destruction of 'recipient' cerebral areas but to a 'total blocking of attention.' At any rate, in view of the fact that in a very general way sensory defects have been traced back to a

pathological functioning of attention, it is not surprising that the fragmentary perception found in some pathological cases has been 'explained' by disturbances of attention.

It seems to us, however, that pathologically disturbed attention can neither satisfactorily explain fragmentary perception nor fragmentary eidetic imagery. Assuming, *e.g.*, that the 'normal' functioning of attention, as it occurs in 'normal' processes of perception, is pathologically disturbed in fragmentary perception, does not give us any new insight. It merely means a re-description of the fact that 'normal' and 'pathological' perception are different in many ways. Nothing is gained by explaining fragmentary eidetic imagery by a 'perverted' attention, since it raises immediately the question as to how this kind of attention came about. To be sure, so long as 'attention' is merely a vehicle for more adequately characterizing certain aspects of the data descriptively we do not object to it. As an approach to an understanding of the phenomena of fragmentation, it is not possible either to link them up, as Pötzl does, with Euler's relation, Fechner's law, Planck's entropy formula, and Talbot's law. At present such considerations are of the nature of pseudo-quantitative speculations. Still the question remains as to the best approach to an understanding of fragmentation.

To begin with, we must realize that speaking of spatio-temporal fragments is justified only in comparison with the *units*, *i.e.*, the objects, normally seen or reproduced. The term 'fragments' is misleading in so far as it seems to suggest some definite knowledge concerning the relation between the functions involved in fragmentation and the functions involved in perceiving and reproducing objects 'normally.' Such knowledge is not at hand. Let us illustrate this point. The statement found in the literature of brain pathology that the visual system consists of five part-systems: (1) the brightness system; (2) the color system; (3) the form system; (4) the system mediating the perception of movement; and (5) the system mediating the perception of direction, is justified only if it is taken to mean that there are part-systems which may be independently disturbed. We may

have brightness perception without color perception, color perception without perception of form, etc., but we cannot assume that the functions underlying the 'normal' visual response are nothing but an addition of the functions thrown successively into gear when, in the recovery, *e.g.*, of a cerebral case, one part-system after the other begins functioning. The data available are simply not sufficient to support such an assumption. Of course, it is very likely that the functions operative in the visual part-systems are somehow operative, too, in the normal visual response. But we do not know anything about the *how*. We may conclude, then, that we do well to state our ignorance concerning fragmentary eidetic imagery in an equally clear manner: we are not only in the dark concerning the factors responsible for fragmentation, but we cannot even make a definite statement concerning the way the functions involved in fragmentary reproduction are related to 'normal' reproduction. Any such statement must be considered an hypothesis.

The following statements contributing to an hypothesis merely purport to characterize the nature of fragmentation in a more precise way. The facts involved in fragmentary reproduction suggest different visual levels. We must assume a level on which the splitting-up into spatio-temporal fragments takes place; that means, a level with laws of its own and a behavior of its own. The dynamics on this level is not interfered with by *external* stimulation or by *intentional* acts. The tension exerted externally or intentionally remains ineffective. We believe that this fact must be the point of departure for any theory, and not the fact that intentional acts or external stimuli do exert, as our above summary of the data shows, an influence on the functional level underlying fragmentation.

The important point is not that a black 'spot' turns into a 'dog' and that after a period of 'concentration' a word or a number is reproduced. The important point is that there are cases in which the reproduction of words does not begin with recalling that the word consisted of capitals, or was written horizontally, or had about so-and-so many letters,

but begins with the dot of the 'i' or with a part of a 'u,' or with the appearance of one of the letters in the word. The important point in the reproduction of a number is that it may start with the *parts* of one figure as, *e.g.*, the upper part of an '8' or the lower part of a '6.'

It is necessary to recall the 'errors' commonly made in the reproduction of symbols tachistoscopically exposed and other experimentally determined forms of reproduction in order to appreciate the distinctiveness of fragmentary reproduction. If something similar to it is ever observed, the assumption is easily made that 'attention,' during the exposure of the stimulus object, has purposely dissected the stimulus material in a very arbitrary way. But in the findings reported here, such a possibility seems to be out of the question since many individuals do not even remember the stimulus material exposed, not to mention a specific mode of attentional dissection during the exposure. Still the fragmentation we get seems to be just that kind of fragmentation which, as Köhler (**12**) seems to believe, never occurs in 'natural' or 'naïve' seeing. Considering the stimulus material used, the instructions employed, in short, the experimental conditions under which the results were obtained, it must be said that the fragments reported on are neither 'natural' sub-wholes nor can they be accounted for by a reliance of the subject on the general characteristics of the stimulus, on its *Gestalt* qualities, generally believed to be of paramount importance in reproduction.

It may be said, of course, that on the level of fragmentation, highly organized wholes, complex visual configurations, are 'disintegrated.' But it seems to us that in general the disintegration-products of sensory wholes, as we find them in certain 'organic' cases or in the perception of quickly exposed objects or as a result of a considerable lapse of time after the exposure of the object, are different from the very specific and definitely circumscribed fragments we have reported.

To cite various facts: the 'fragments' of Schn . . ., a patient of Gelb and Goldstein, are colored or colorless spots in a certain distribution (**3**). The 'Beziehungsbewusstsein'

of cerebral patients as it manifests itself in the presence of pathologically modified sensory material is apparently never destroyed. We have found that the (visual) larger-than relation in normal monkeys is effective in spite of changes in absolute size, color, shape, etc. But in collaboration with Lashley we have also found that different cerebral lesions do not destroy the efficacy of larger-than relations. A larger-than relation, to be sure, implies the efficacy of supra-local properties. In general, there can be no doubt that in perception and reproduction certain supra-local properties are highly important. The eidetic fragments we have referred to seem to suggest the functional dominance of *local* properties. The importance of supra-local characteristics or *Gestalt* qualities really means (1) the non-importance of certain characteristics of the stimulus for reproduction—a square-object may lose its color, yet it remains a square; (2) the importance of certain 'crucial' characteristics—a square represented by four dots cannot lose one of these dots since a triangle will result.

The main point concerning fragmentary reproduction is not that *non-important* characteristics of the stimulus are reproduced, but that the *crucial* characteristics themselves suffer fragmentation; that, so to speak, squareness is reproduced by reproducing triangularity first. Whenever we have conditions of this kind—and we hypothetically assume such conditions in fragmentary reproduction—squareness is, briefly speaking, dependent on triangularity. That means that on the functional level *fragmentary reproduction must be viewed as an emission of Gestalt quanta instead of a process representing the correlate of Gestalt qualities.* It seems to us that an analysis of the facts justifies the introduction of Gestalt *quanta.* The facts also show that in fragmentary reproduction Gestalt qualities cannot be ruled out and that even a 'dot' is *gestaltet*—hence *Gestalt* quanta. There is no doubt that the essential nature of the functions underlying those aspects of fragmentation which to us represent the most significant ones in formulating an hypothesis, is more adequately dealt with by the concept of Gestalt *quanta* than by

the concept of Gestalt *qualities*. We assume that in eidetic and pathological vision as well as in certain visual phenomena produced by drugs (and perhaps even in normal vision) 'squareness' may involve 'triangularity' and that experiments can be formulated for demonstrating the significance, not of a quantum psychology, but of *Gestalt quanta* in perception and reproduction.

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DISCUSSION

A NOTE ON PAVLOV'S CONCEPTION OF THE UNCONDITIONED RESPONSE

An examination of the work of Pavlov which is contained in 'Conditioned reflexes' (1927), and in 'Lectures on conditioned reflexes' (1928), indicates that the fact of early conditioning in what he called the 'unconditioned reflex' has been overlooked. This term is applied to 'inborn' reflexes by Pavlov, and yet the first mentioned volume states explicitly that only adult or near adult animals were used in the experiments, animals the past histories of which were not known. Likewise, such of Pavlov's students who experimented with children (*e.g.*, Krasnogorski) did not study conditioning of infants but used cases which, judging from Watson's and Jones' experiments on infants, had been conditioned in many ways. In short, Pavlov and his students give no indication of having studied adequately the nature of the unconditioned reflex, nor how it is related to the conditioned reflex, since they never investigated the first responses of any of their subjects.

What Pavlov has done proves that conditioned reflexes vary in strength and that one conditioned stimulus can be transferred to another reflex. He seems also to be correct in stating that a reflex which can be used in conditioning another is stronger than one which cannot be so used, but he has been incorrect in attaching the term 'unconditioned' to any of these responses until he has investigated the new-born organism of the type used for experiment.

The problem of conditioning of innate responses is one of the moot points of the instinct controversy. Kuo (PSYCHOLOGICAL REVIEW, 1924) has pointed out that perhaps the use of a scale of perfection of response would be of advantage in eliminating the problem of heredity as opposed to environment, and, at the same time, lead to more numerical measurements of activity. Such a method would make the problem of conditioning assume a mathematical value on a scale. On such a scale, Pavlov's unconditioned reflex may or may not be at the point which signifies perfect success at the first trial, and it may or may not be found to vary in different individuals and species.

One distinct danger of the use of the term 'unconditioned' for what is not definitely known to be present at birth is the consigning to the more mysterious what is less mysterious, a practice to which all are too liable. But a more serious error is evidenced by those who accept Pavlov's entire writings uncritically—as a sort of physiological Bible. Moralizing minds may be especially victimized, but otherwise careful scientists also fall into this error unconsciously, due to the fact that a man's prestige offers a ready support.[1]

[1] L. T. Hobhouse (Vol. I, p. 168) and C. S. Sherrington (Vol. IV, p. 6) in the new edition of the *Encyclopedia Britannica* have briefly but adequately mentioned the essential nature of the unconditioned reflex.

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